Natural Science

A Monthly Review of Scientific Progress

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NOTES AND COMMENTS.

Integration in Science.

UNDER this title Sir Michael Foster delivered a stirring address to the Yorkshire Naturalists' Union last December, and the address is now reprinted in full in *The Naturalist* for July. Its object is to consider how Naturalists' Societies may be used to check the tendency of biological science to disintegrate into separate and distinct sciences, and to show how far that disintegration has already proceeded, and how great the need for integration. Sir Michael compares the Temple of Science to that earlier erection which men are said to have built on the plain of Shinar. Both buildings seem to have the same consequences, in that, as they rise, the builders cease to understand one another's tongues. What then shall the modern workmen do to prevent the fate of their prototypes becoming their fate also? Has not the confusion of tongues already proceeded so far that the workmen are scattered and the building delayed? As Sir Michael points out, not only have physicist and chemist learnt to speak a language unintelligible to botanist and zoologist, but worse still, the erstwhile zoologists are split into anatomists, physiologists, and systematists, each of whom uses a tongue foreign to his brother. The extension of the examination system has aggravated the evil, until to many a "zoologist" the animal form is seen only through "the long vista of a lengthy ribbon of gorgeously stained microtome-cut sections of exquisite thinness." That much of this is the necessary consequence of the division of labour and the progress of knowledge cannot be denied, nor can we forget that the "outcome of the deepest, most far-reaching biologic inquiry has been the rehabilitation of the naturalist of old," yet the reality and extent of the evil can hardly be overestimated. Sir Michael is of opinion that there is little hope of remedying it by an appeal to the schools, but he thinks that it is the special function of Naturalists' Societies to assist in the process of integration, and to teach the academic neophytes something of the meaning of the word naturalist. The moral is so excellent that it seems worthy of the attention of societies other than that to which it was addressed.

Women and the Learned Societies.

At the recent International Congress of Women in London, Mrs. Farquharson of Haughton, in the course of a paper on the work of women in biological science, drew attention to the fact that at least three of the large scientific societies still refuse to admit women to their full fellowship, however fully qualified they may be. These three societies are the Royal, the Linnean, and the Royal Microscopical. Of these the Royal Microscopical admits women to its membership, but refuses to permit them to attend its meetings, while the two other societies entirely refuse membership on any terms. Mrs. Farquharson dwelt upon the hardship thus entailed upon women in special cases.

British Botany.

That much still remains to be done in the field of British Botany—at any rate among the lower plants—is evident from papers which have recently appeared in the *Journal of Botany*. In the May number of the *Journal*, Mr. Gepp notes the occurrence of no less than four aquatic fungi, hitherto unrecorded from Great Britain, which were found growing on a broom-handle floating in a reservoir near Shrewsbury. These fungi belong to the genera *Achlya* and *Apodachlya*, of the family Saprolegniaceae; and there is little doubt that a careful study of the native members of this group, on the lines suggested by the writer, would result in other interesting finds.

The July number of the same Journal contains a description and figure of a fresh-water Alga, which forms not only an addition to the British flora, but a variety new to science. It is a filamentous green Alga allied to the common *Cladophora*, and forming, like the latter, masses of tangled green threads, but of finer consistency and a brighter green. It belongs to the genus *Pithophora*, the history of which is of some interest. The genus was founded by the Scandinavian botanist Wittrock, on a plant which appeared some years ago in the water-lily tank at Kew, and had presumably been introduced from the Amazons along with the lilies. Wittrock subsequently described several other species from various parts of the world. The original one has long since disappeared from Kew, and has not been found elsewhere; but another, the subject of the communication, has recently appeared in the Reddish Canal, near Manchester. This canal is a classical locality, having supplied a new *Chara*, and also become the home of an aquatic monocotyledonous flowering plant, *Najas graminea*. The latter is widely spread in the tropics of the Old World, and has also long been known from Northern Italy, where it is generally supposed to have been brought

from Egypt with rice. It is suggested that its presence near Manchester is due to an introduction of the seeds along with Egyptian cotton, and this view is supported by the fact that the Manchester plant resembles Egyptian specimens in a certain anatomical detail of the leaf-structure. The new Alga was growing attached to the stem and leaves of the *Najas*, and may have been similarly introduced; but, so far, the genus *Pithophora* has not been recorded from North Africa.

Polemics and a Parasite.

THE *Zoologischer Anzeiger* for July 3 contains an article by Professor W. M. Wheeler entitled "J. Beard on the Sexual Phases of Myzostoma" (pp. 281-288), which is a fine example of polemical discussion. We all like a fair fight, even if we won't admit it; and perhaps these zoological tilts are like the combats of male spiders in this, that neither party is wounded. Wheeler criticised Beard, and Beard criticised Wheeler, and the bystanders were edified; and we cannot but say that the edification continues as Wheeler returns to the charge. Our only doubt is as to the wisdom of using words that have a moral connotation, words like "garble" and "misrepresent," which we see in the paper before us. A more philosophic note is struck when Mr. Wheeler expresses the hope that "continued controversy may induce some student (we omit the adjective conscientious) who has an opportunity of working at the Naples Station or at the French or Japanese sea-side laboratories, to undertake a renewed study of the reproductive organs of the various species of *Myzostoma*."

But what is the dispute about? Beard holds that *M. glabrum* is dimorphic, the species being represented by hermaphrodite individuals and by dwarf complemental males. The latter are dorsicolous, that is, they are attached to the dorsal surface of the large hermaphrodite individuals which in turn adhere to the peristome of *Antedon rosacea*.

From a comparative study of several species representing the morphological extremes of the genus *Myzostoma*, Wheeler concluded that *M. glabrum* is monomorphic, each individual being from the first hermaphrodite, *i.e.* possessing both ovaries and testes, and being like other members of the genus (notably *M. cirriferum* and *M. alatum*) protandrous, then hermaphrodite, and ultimately more or less hysterogynic. "In other words, the functional male phase (Beard's complemental male) passes into the functional hermaphrodite phase as soon as the first ova mature, and the functional female phase begins with the atrophy or disappearance of the testes. The cysticolous and endoparasitic species of the genus tend towards a condition in which the functional male and female phases overlap but little, thus exhibiting only a brief functional hermaphrodite phase (*M. eremita*), or these

phases no longer overlap and thus present two well-marked periods of sexual maturity, one male and the other female (*M. pulvinar*)." This Mr. Wheeler regards as a simpler and more satisfactory "explanation" (or rather description) of the sexual peculiarities of *Myzostoma* than has been offered by Beard or any other author. He proceeds to criticise Beard's critique, and ends up by expressing the hope that "every fair-minded zoologist will be convinced that the complemental male of *M. glabrum* is one of those tenuous and fanciful creations for which one could have wished that euthanasia, that silent death so becoming to pet speculation when they have ceased to afford either amusement to their originator or edification to their readers." The temperature of Chicago is high!

Life High and Low.

A SUMPTUOUS French translation has been published of an essay by Prof. A. L. Herrera and Dr. D. Vergara Lope, on life on the high plateaux[1]—an essay which gained honourable mention and a silver medal in the competition for the Hodgkins prize of the Smithsonian Institute in 1895. After a general discussion of plateaux, the authors consider the vertical distribution of plants and the adaptations exhibited by those living at high altitudes. They then pass to the vertebrate animals composing the plateaux-fauna, and show that here also special adaptations may be detected, especially perhaps in the function of respiration. Man's life on the heights is then considered, and many facts are cited and suggestions offered as to the therapeutic value of a residence on the plateaux. The work is laboriously erudite and carefully planned, and will be a welcome addition to the consulting library of biologist and physician alike. Against the old theory that life at high altitudes is too difficult both for man and beast to be healthful, and that it brings about degeneration of body and mind, the authors argue most strenuously. Their central thesis is that plants, animals, and man may become acclimatised to high altitudes, and live a life of full vigour "obeying the eternally true law: *Semper ascendens.*"

It is a far cry from the Mexican plateaux to thirty fathoms below the Eddystone lighthouse, but the naturalists' problem is the same: how are the organisms adapted to the peculiarities of their environment? Mr. E. J. Allen, director of the Plymouth Laboratory, has been investigating for some years the distribution of the fauna on the sea-bottom along the thirty-fathom line from the Eddystone Grounds to Start Point, with the particular object of ascertaining and, where possible, explaining the changes which take place in the animal

[1] "La vie sur les hauts plateaux," pp. 790, 18 tables, numerous plates. Mexico, 1899.

population when the nature of the bottom deposit changes. It has been a laborious piece of work, executed with patient carefulness, and the results though not startling are certainly valuable.[1]

Since the principal object of the investigation was to study the relation of the fauna to the bottom-deposit, the area selected for examination was so chosen that the general physical conditions were uniform apart from the nature of the deposits, and the amount of disturbance of the bottom water by the action of waves was relatively small. The chief results to be gained by carefully scanning the numerous tables—the drawing up of which must have meant a large amount of work—relate to the suitability of certain kinds of ground for certain kinds of animals, but apart from this the memoir is also interesting because of the numerous notes on the habits of the animals and for its analysis of the environmental conditions.

The physical conditions, the variations of which influence the life of bottom-living species, are capable of definite statement, and for the most part of accurate measurement. They are—

1. The constitution of the sea-water.
2. The nature of the bottom-deposit.
3. The movements of the water, due to
   (*a*) wave action,
   (*b*) currents,
   (*c*) tides.
4. The temperature of the sea-water.
5. The pressure, varying with the depth of water.
6. The amount of light which penetrates to the bottom.

The external biological conditions influencing the distribution of any bottom-loving organism, due to the existence at the same time of other living organisms, are often of a complicated nature.

1. One organism may exert an advantageous influence upon another.
   (*a*) By serving as its food-supply;
   (*b*) By serving as a fixed base to which it may attach itself;
   (*c*) By serving as a movable base, and thus extending the area over which a fixed organism can collect its food-supply;
   (*d*) By bringing supplies of food to the other organism as well as to itself, either by setting up a current, or in some other way;
   (*e*) By affording the other organism means of protection or concealment from its enemies.
2. One organism may exert a disadvantageous influence upon another.
   (*a*) By preying upon it;

[1] "On the Fauna and Bottom-deposits near the Thirty-fathom Line from the Eddystone Grounds to Start Point," *Journ. Marine Biol. Ass.* v. June 1899, pp. 365-542, 15 charts and 7 tables.

(*b*) By fixing upon it in such a way as to destroy it;
(*c*) As a competitor for a limited food-supply, or for a limited amount of fixing space.

3. The biological conditions by which the organisms on any particular patch of ground are influenced depend not only upon the organisms living on that ground itself, but also upon the nature and abundance of the organisms living upon neighbouring grounds.

We have quoted the above analysis because it seems to us admirable, and indicative of the careful manner in which Mr. Allen has dealt with his problem. And although the research has a less obvious practical outcome than that on plateau-life, with which we have coupled it, this justification is not awanting, for it helps towards an understanding of the local distribution of food-fishes.

As Regards Protoplasm.

THOSE acquainted with Prof. E. B. Wilson's work entitled "The Cell in Development and Inheritance" will remember that he is no optimist, and will not be surprised to find him saying in a more recent deliverance (*Science*, x. 1899, pp. 33-45, 4 figs.):—"If we except certain highly specialised structures, the hope of finding in visible protoplasmic structure any approach to an understanding of its physiological activity is growing more, instead of less, remote, and is giving way to a conviction that the way of progress lies rather in an appeal to the ultramicroscopical organisation and to the chemical processes through which this is expressed." He starts in his lecture with a familiar object—the egg of the sea-urchin—and defines the problems suggested by it: (1) What is the actual structure that gives the appearance of a meshwork? (2) How faithfully does the preserved structure, as seen in sections, reproduce that existing in life? (3) What is the relation of the astral systems to it? (4) What is the finer structure and origin of the meshwork? (5) Can this structure be taken as typical of all protoplasm; and if not, what is its relation to other forms of protoplasmic structure? And incidentally, still another interesting question arises: Is it possible to identify any one of the three visible components—granules, continuous substance, ground-substance—as the living substance or protoplasm proper, as distinguished from a lifeless metaplasm, and, if so, what are its structural relations?

To propose dogmatic answers to these questions would be at present absurd, and Professor Wilson is of no such mood. He has, however, specialised in cytological work, and his conclusions are therefore of value to less intimately initiated workers.

As to the nature of the meshwork he concludes that in the resting

cell it is in reality an alveolar structure — an emulsion — such as Bütschli has described. The living stuff of an Echinoderm ovum is in the form of a fine emulsion consisting of a continuous substance in which are suspended drops of two orders of magnitude and of different chemical nature, the larger drops determining the primary alveolar structure as described by Bütschli, the smaller drops determining the secondary or finer alveolar structure as described by Reinke. As to the astral rays in the sea-urchin egg and elsewhere, they involve a radial arrangement of the alveoli, but they involve more, namely, definite fibrillae which grow by progressive differentiation out of the general cytoplasmic meshwork.

The phrasing of the last sentence suggests a more general conclusion — "that alveolar, granular, fibrillar, and reticular structures are all of secondary origin and importance, and that the ultimate background of protoplasmic activity is the sensibly homogeneous matrix or continuous substance in which those structures appear." Not that the author puts his finger upon this, so to speak, and says this is *the* living matter, for "in its fullest meaning the word living implies the existence of a group of co-operating factors more complex than those manifested by any one substance or structural element in the cell, nevertheless, we are perhaps justified in maintaining that the continuous substance is the most constant and active element, and that which forms the fundamental basis of the system, transforming itself into granules, drops, fibrillae or networks in accordance with varying physiological needs." Thus we are led to the conclusion that the physical basis of life is in the invisible organisation of a substance which seems to the eye homogeneous. Beyond this, as far as morphological aspects are concerned, all is hypothesis, and the form of hypothesis which Professor Wilson favours is "that the homogeneous or continuous substance may be composed of ultra-microscopical bodies, by the growth and differentiation of which the visible elements arise, and which differ among themselves chemically and otherwise, as is the case with the larger masses to which they give rise."

The Darmstadt Museum.

Although the new building of the Grossherzogliche Museum at Darmstadt is unfinished and untenanted, the plan of the zoological portion has been carefully worked out by Dr. G. von Koch, the director, and some idea of its main features can be gained from his programme and from the newer cases in the old museum.

In the "Schausammlung" or show collection intended for general instruction, there is of course a systematic series, but prominence is given to cases showing things more or less as they are in nature or

grouped to illustrate some particular fact or adaptation. Thus we see a beech wood in winter with its withered leaves, squirrels, and woodpeckers; the bank of a stream with its wagtails, kingfishers, and other tenants; a tree with distinctive nests at the various levels, and so on.

Other cases—more difficult to work out naturally—are beginning to illustrate geographical distribution, so that he who runs—and such is too often the museum pace—may almost read. The posing of many of the birds, such as the albatross, in flying attitude; the juxtaposition of the stuffed creature and its skeleton (as in the case of *Ateles geoffroyi*); the arrangement of lenses over selected corals; the models showing musculature in natural size, *e.g.* of the elephant's skull and fore-limb, and other features, struck us as we walked through, and lead us to look with expectation to the opening of the new museum. Dr. Koch evidently believes in keeping the detailed collection for workers in a form which will be convenient to the student and will save the laity from embarrassment, and in making each exhibit of the so-called show collection really teach something.

An Annelid from the Devonian.

THE lamentable condition of fossils found in the Devonian rocks of the south coast of Cornwall makes a communication by Mr. Upfield Green to the Royal Geological Society of Cornwall of more than ordinary interest. This consists of a brief record with figures of the impression of an annelid to which he has given the name of *Nereitopsis cornubicus*. The specimens come from the slates of Polruan, Polyne, and two unknown localities, and are four in number. They are identical in structure, and are certainly impressions of different individuals of the same species. 'As Mr. Green has not ventured to describe them, it may be well to offer a few remarks on the original specimens, which are faithfully represented by the figures of life size. From the central rod, now represented by a hollow, and which shows traces of segmentation, spring pairs of impressions of parallel striae, the distal end of each of which terminates in a $>$ shaped point. Each pair of impressions increases in size from the tail towards the head (not seen in any of the specimens). The tail appears to have a swollen and tuberculated aspect, but is obscure. Such in few words is a description of these curious fossils, which have been illustrated and published in the hope that better material may be forthcoming now that attention has been drawn to them. The originals are in the Museum of the Royal Geological Society of Cornwall at Penzance.

Cultivation of the Vine for Wine in Essex.

A QUESTION relative to the above heading was asked in the *Essex County Chronicle* for Dec. 9, 1898, and has produced a paper on the subject in the *Essex Naturalist* (Jan.-March 1899) from the pen of Mr. Miller Christy. This paper, which is of considerable interest, deals with the matter historically, and collects together a great deal of valuable information. For instance, no fewer than eight records of vineyards in Essex occur in Domesday Book, and other records occur for 1130, 1252, 1303, 1380, 1540, 1667, etc. Wine was produced, according to these records, in 1086, 1130, and 1667, the produce of the latter year being mentioned by Pepys as grown at Walthamstow. Reference is made to the place names, and to hop-growing, and to the fact that the vine is largely grown at the present day for the sale of the grapes themselves, rather than for the wine the grapes might yield.

Did Palaeolithic Man Inhabit Scotland?

IN a brochure by the Rev. Frederick Smith of Cromlix, entitled "Some Investigations into Palaeolithic Remains in Scotland" (a reprint from the *Proceedings of the Philosophical Society of Glasgow*, read 30th November 1898), the author claims to have discovered palaeolithic implements in many localities throughout Scotland, including the valleys of the Forth, Tay, Earn, Allan, Dee, and Don (Aberdeen), as well as the Clyde estuary.

That such implements have not been hitherto recognised in Scotland is, according to Mr. Smith, due to the fact that "the searchers were looking for the wrong thing. The accepted forms being of flint, flint specimens were sought in Scotland; or, on the supposition that other materials than flint might have been used, specimens of equally fine form and elaboration were expected. But no flint exists in Scotland; hence flint specimens could not have been anticipated." No objection can be taken to the logic of the above statement, but it is equally certain that if palaeolithic man did not inhabit Scotland, as has hitherto been assumed, the products of his hands need not be looked for. With regard to Mr. Smith's reported discoveries, the main question which has to be determined is, whether the objects are, or are not, of human workmanship. Should this be decided in the affirmative the next step would be to ascertain if they were actually found in circumstances which would lead us to regard them as the handiwork of Palaeolithic Man? On both these points the author is very confident of a favourable verdict. He tabulates his results as follows:—

(1) "Angular—*i.e.* unrolled—stones, in shape similar to the flints

of the Somme, but wanting the characteristic flaking, were found in the soils of the higher areas of the lower Tay valley, but were entirely absent from those of the 50-feet and lower terraces."

(2) "Similar stones found in Kaims and the most ancient river deposits, but more or less rolled or water-worn."

(3) "These stones entirely absent, under ordinary circumstances, in recent river deposits; if present, so completely water-worn as to be practically unrecognisable."

There exists, no doubt, a borderland, in which it would be difficult to distinguish natural productions from the ruder works of man; but so long as this indefiniteness characterises Mr. Smith's specimens, no archaeologist would be justified in concluding from them as to the presence or absence of man in the district. Until this problem is settled we need not inquire into the merits of the subsidiary one. For the clear, methodical, and terse manner in which Mr. Smith has laid the facts before the public he deserves a word of encouragement, but we cannot say that he has proved his case.

Insects and Tobacco.

THE Year-Book of the U.S. Department of Agriculture for 1898 contains an interesting paper by Dr. L. O. Howard on insects injurious to the tobacco plant. It is remarkable that this plant, though native in North America, is less subject to insect ravages than are cereals and other imported crops. The most destructive of the enemies mentioned here is a small "flea-beetle," *Epitrix parvula*, which eats holes in the leaves, and renders them liable to further damage through the entrance of fungus-spores. The caterpillars of two large hawk-moths and of several noctuids, including species so familiar to British entomologists as *Agrotis saucia* and *Heliothis armigera*, are also noticed. Even when prepared for consumption in another way by vertebrate admirers, tobacco is still sought after by hungry arthropods; the "cigarette beetle," *Lasioderma serricorne*, bores into all kinds of stored tobacco. "An entomological acquaintance," writes Dr. Howard, "insists that he buys infested 'short cut' by preference, both because he can get it cheaper, and because the bodies of the insects impart a distinct and not disagreeable flavour to the tobacco. He admits, however, that it is a cultivated taste."

Ichthyosaurus at Home.

One of the shortest cuts to a realisation of *Ichthyosaurus* is a journey to the Museum in Stuttgart. It may be that the Saurian's rehabilitation is still caviare to the general, but there are many accessory attractions by the way. The Stuttgart Museum—the Naturalien-Cabinet as they call it—is indeed a treasure-house for students of palaeontology, whether they are interested in tertiary mammals or the teeth of *Microlestes*, crustaceans or Steinheim molluscs, Labyrinthodonts or Saurians, and it is said that the thicket of mammoth tusks from Cannstadt has proved so impressive that it is mentioned in Baedecker, which surely means an Ultima Thule of fame.

The museum as a whole is painfully suggestive of what museologists call "the fat boy," except in this respect that it seems in no wise somnolent. But it puzzles the inquisitive visitor to imagine where a single additional specimen could possibly be stored. The most ingeniously crowded cases of "Vermes," for instance, are positively heartrending, and one feels that a few more exchanges would leave only the labels visible on the ascending staircase of bottles.

Among the striking features may be noted the extraordinarily rich series of Pheasants and Birds of Paradise; the fine representation of the Württemberg fauna, including that strange phenomenon—Rattenkönig—of many rats entangled by their tails, and with a wealth of duplicates, *e.g.* of *Pelias verus*, which must surely embarrass anyone but a student of variations; a skilfully displayed set of insects injurious to herbs and trees; besides various fascinating rarities like the Great Auk.

Yet *the* feature of the collection is doubtless the series of Saurians (in the wide sense) on which Dr. Fraas—one of the custodians of the museum—has worked with so much success. It was among these that we recently spent two happy forenoons, and it was the wealth of species and individuals of Ichthyosaurus—from one measuring twelve metres in length to a little foetus within its mother—which suggested the title of our note, written not for the learned palaeontologist at home, but for the amateur naturalist abroad, in the hope that among the thousands of English visitors who pass annually through the charms of Stuttgart, this may possibly arrest some to enjoy the glimpse into an ancient world which the palaeontological museum affords. There are of course many richer collections, but it will be hard to find one equally rich of which it can be said that all the treasures are local. Perhaps even the Stuttgarters themselves are but dimly aware that the Naturalien-Cabinet is a much more marvellous treasure-house than even the wonderful Moorish Palace of which they are justly proud. Similarly, there are but few elect Dundonians who have any notion of the wealth of Prof. D'Arcy Thompson's collection

in University College. Our point, however, was that to realise Ichthyosaurus, to see it disporting itself with its flukes, to verify its dorsal fins, to inquire into the contents of its stomach, to peer even into its oviduct, one must go to Stuttgart and sit at the feet of Fraas.

A Note on Zoos.

AGAIN and again it has been remarked that zoological gardens flourish on the continent in towns whose population is less than that of British centres in which the institution of a "Zoo" would be regarded as foredoomed to failure. The reasons for this are doubtless manifold:—the treacherous British climate is largely to blame; we are given to take our pleasure sadly; there is the little item of delectable uninjurious beer with which British brewers still leave us unprovided, and so on.

The pros and cons have been often discussed, and we have had some opportunity of considering them. Our verdict is that a "Zoo" would flourish and pay in Edinburgh, for instance (where the project has been recently discussed with more or less vague enthusiasm), just as well as in Stuttgart, if only a company would select a scientific person with brains to run it.

After visiting the garden in Frankfurt, which is in some ways almost luxurious in its wealth of exhibits, we were glad for our country's sake to see the little Nil-Garten at Stuttgart. For Edinburgh all at once to start a zoological garden on the scale of the Frankfurt one is as unlikely as that there should be an independent Edinburgh Antarctic Expedition; but that a company of enthusiastic Edinburgh naturalists and business men should not be able to run as good a garden as there is in Stuttgart is absurd.

So far as we could gather, it seems to be "run" by one man, and there were few irrelevant attractions. Yet the garden was an interesting one, with its *Echidna*, a very fine *Myrmecophaga jubata*, a sloth, an orang, a chimpanzee, the usual galaxy of monkeys, a fair sample of carnivores and ungulates, a lot of quite happy birds, a great somnolent giant salamander and silurus, and so on.

There was not perhaps anything new to the expert naturalist, but there was enough for even his observation for an hour or two. The collection seems to have started with monkeys, but it has broadened out, and it is at once a credit to the town and an example to others who might go farther for suggestion and fare worse! One thing, however, a visitor to the Stuttgart garden must feel, that without a good water-supply a thoroughly successful and beautiful Zoo is impossible.

ORIGINAL COMMUNICATIONS.

The Original Rock of the South African Diamond.

By Professor T. G. Bonney, D.Sc., LL.D., V.P.R.S.

In 1867 the first diamond was discovered in South Africa, one having been found in some gravel from the Orange River. Three years afterwards it was obtained in a peculiar deposit of a yellowish colour, like a rotten, rather saponaceous shale, about 15 miles away from the stream and near the present site of Kimberley. There was a rush to the spot, and excavations were soon opened. For some time the mining places were only four in number, and near Kimberley; a fifth was afterwards added, but all of them lie within a circle of about $3\frac{1}{2}$ miles in diameter. Since then similar deposits have been found elsewhere, and the Newlands Mines, in West Griqualand, to which I shall more especially refer, are about 42 miles to the N.W. of Kimberley. The diamantiferous "yellow ground," as the miners called it, was found, as it was worked downwards, to change gradually into a rather more coherent rock, of a dull dark green-blue colour, named "blue ground"; this became more solid as the workmen followed it downwards, till at a depth of 1200 to 1400 feet it is nearly as consistent as a limestone.[1] In this matrix the diamond occurs,[2] together with a number of other minerals, such as garnets (chiefly pyrope), olivine, pyroxenes (including enstatite, chrome-diopside, and smaragdite), a brownish mica passing locally into a chlorite, ilmenite, and magnetite, with small fragments of zircon and kyanite.[3] The ferro-magnesian minerals are more or less serpentinised, and the pyropes are often surrounded by a kelyphite rim, much of it consisting of brown mica. The diamonds, it may be added, are often found, by their

[1] I believe that 1800 feet has been reached in the De Beers Mines, but I have not heard whether the hardness of the rock has materially increased; probably it has not.

[2] According to the De Beers Consolidated Mines Report, 1889-90, the average yield in that mine is from $1\frac{1}{4}$ to $1\frac{1}{3}$ carats per load (about 1600 pounds); the Kimberley is much the same. In Bulfontein and Du Toit's Pan it varies from $\frac{1}{8}$ to $\frac{1}{4}$ of a carat per load.

[3] See Lewis, "Genesis and History of the Diamond," for a very full history and account of the minerals, large and small.

anomalous optical character, to be in a condition of strain, and they are sometimes only fragments of crystals.

The matrix, in which the above-named minerals are rather irregularly scattered, consists of serpentine, somewhat fragmentary in aspect, mixed with about 16 per cent of a carbonate—calcite or dolomite, granules of iron oxide and perovskite; sometimes tiny flakes of brown mica—apparently of secondary origin—are generally disseminated. To some investigators the rock seems to be porphyritic, to others brecciated, several of the minerals looking rather rounded. Angular rock fragments—shales, grits, diabases, and the like (the first of these sometimes apparently a little altered)—are also present, though in variable quantity. The country rock is a shale, often dark, interbedded with hard grits, and associated with flows or sills, and with dykes of igneous rocks, mostly basalt or diabase. Dykes also occasionally cut the diamantiferous rock. The latter occurs in pipes which bear a general resemblance to volcanic necks. These vary in size, the largest, named Du Toit's Pan, being about 45 acres in area.

This very brief sketch of the circumstances under which the South African diamonds have been hitherto found may suffice for our present purposes, since so much has now been written on the subject.[1] The facts which have been briefly summarised have received very diverse interpretations, though all admit that the rock has been considerably affected by secondary mineral changes, which have been brought about, in all probability, by the action of heated water. Some writers, however, maintain that the rock is a breccia, and that the diamond, like the garnets, pyroxenes, olivines, etc., was formed elsewhere, the parent rock or rocks having been shattered by some form of explosion. Others, while taking the same view as to the character of the blue ground, believe that the diamond was formed *in situ*, probably by the action of highly heated water (under considerable pressure) on the carbonaceous material of the country rock (Karoo shale[2]). Others, again, agree with the late Professor Carvill Lewis in regarding the "blue ground" as a serpentinised and otherwise altered peridotite of somewhat peculiar form. For this he proposed the name Kimberlite, thus defining it "a porphyritic volcanic peridotite of basaltic structure, or, according to Rosenbusch's nomenclature, the palaeovolcanic 'Erguss form' of a biotite-bronzite-dunite, being an olivine-bronzite-picrite-porphyrite, rich in biotite . . . it is a rock *sui generis*, dissimilar to

[1] I think it needless to attempt a bibliography. The earlier more important papers, with some which cannot be so designated, will be found in Carvill Lewis's "The Genesis and Matrix of the Diamond," 1897. Some of later date are mentioned in my paper on "The Parent Rock of the Diamond in South Africa," read to the Royal Society on 1st June of this year. The classic paper of Professor Maskelyne and Dr. W. Flight (*Quart. Journ. Geol. Soc.*, xxx. 1874, p. 406) contains the first thorough investigation of the associated minerals, and much information will be found in De Launay, "Les Diamants du Cap," Paris, 1897, and in Max Bauer, "Edelsteinkunde," Leipzig, 1896, both of them most valuable works of reference.

[2] This is referred to the Triassic period.

any other known species. Three varieties of Kimberlite may be distinguished: (1) Kimberlite proper, a typical porphyritic lava; Kimberlite breccia, the same lava broken and crushed by volcanic movements and crowded with included fragments of shale; (3) Kimberlite tuff, being the fragmental and tufaceous portion of the same volcanic rock. These varieties pass by insensible gradations one into another, so that no sharp line can be drawn between them, and all occur together in the same neck or crater."[1] He held that the diamond was produced *in situ*, the basic magma of the peridotite offering so little facility for the oxidation of the carbon.

In this diversity of opinion two points had to be settled before the genesis of the diamond could be determined: (*a*) whether that mineral was authigenous—crystallised on the spot—in the so-called Kimberlite; and (*b*) what was the true nature of that rock. If it were a serpentine, there was then a high probability (though not certainty) that the diamond was authigenous and the date of its birth later than the Triassic period; if, however, the rock were a breccia (produced by some form of volcanic explosion), it was then more probable that the diamond, like many of the other minerals, had been obtained from the shattering of some more ancient crystalline rock.

My connection with this interesting and amicable controversy began in 1891,[2] when, at the request of Professor Rupert Jones, I examined with Miss C. A. Raisin some minerals and small rock fragments which he had received from South Africa. Of the former specimens nothing more need be said since they were those usual in "washings"; but the latter were clearly pieces of a coarse eclogite, consisting mainly of a red garnet and a green augite (that now identified as chrome-diopside); both being minerals found in the Kimberlite. This investigation caused me to pay closer attention to the question, and the circumstances mentioned in the Preface to the "Genesis and Matrix of the Diamond," by my lamented friend Professor Carvill Lewis, led to my undertaking (with the kind aid of Professor Rosenbusch) to see his manuscripts on this subject through the press. But before these reached me I had the opportunity of examining two remarkably well-preserved blocks of the breccia, brought from Kimberley by Sir J. B. Stone, M.P. He kindly presented one of these to me, and a description of it and some other specimens is published in the *Geological Magazine*.[3] I came to the conclusion, as there expressed, that the

[1] "Genesis and Matrix of the Diamond," p. 50. I may add that neither in Professor Lewis's microscopic slices which I studied, nor in the rather numerous collection which I possess, some of them unusually well preserved, have I been able to recognise these three varieties. I have been for some years convinced that the rock was a breccia, and my latest studies (*Geol. Mag.*, 1897, p. 448) proved to me that certain fragments which I had thought might possibly represent a compact peridotite after serpentinisation, must have had quite another origin.

[2] *Geol. Mag.*, 1891, p. 412.

[3] By myself and Miss Raisin, with a prefatory note by Sir J. B. Stone, *Geol. Mag.*, 1895, p. 496.

rock was a true breccia. That opinion was not altered by the study of Professor Lewis's manuscripts, but I thought it possible that his Kimberlite might be represented in certain very compact fragments of serpentinous aspect, the nature of which I had been unable to determine, owing to the want of definite characters and to my own ignorance of what a serpentine formed from a glassy or very compact peridotite would be like. Apart from this possibility, my views on the main question differed from those put forward by my friend. It was, however, my obvious duty to keep the difference of opinion as far as possible in the background, and to endeavour to act as a simple channel for the publication of the views of one who was no longer able to speak for himself. Not long after the book had been published, Sir W. Crookes allowed me to examine a piece of breccia which had been obtained at a depth of 1320 feet, and was in even better preservation than any which I had hitherto seen. About the same time Sir J. B. Stone forwarded to me another set of specimens which he had received from Kimberley. Among these were two or three blocks, in almost as good a condition as that just named, and from an even greater depth, viz. 1400 feet. After study of these [1] I was more than ever convinced that the Kimberlite was a true breccia, formed by the explosive destruction of some coarsely crystalline rocks, such as eclogites and peridotites (including representatives of the sedimentary rocks of the region). I was also able to ascertain the true nature of those fragments which hitherto I had thought might possibly be serpentine of an exceptional character; they proved to be in reality nearer to argillites, but to have undergone certain alterations, in all probability partly from contact action, and partly from water, perhaps at a rather high temperature, and no doubt at a later time. Thus I arrived at the conclusion, that the so-called Kimberlite was not an altered peridotite, but a breccia, in which the diamond, like the olivine, pyroxenes, garnet, etc., was not authigenous, but a derivative from some older rock. This I thought very probably was a peridotite, for an *a priori* argument, as we may call it, which Professor Lewis had used seemed valid, even though he might have misunderstood the nature of the Kimberlite, and his idea that a very basic rock would be the birthplace of diamonds was confirmed by their occurrence in meteoric iron (Cañon Diablo [2]) and their manufacture by Moissan through the intervention of that metal.

Two suggestive discoveries must next be mentioned, of which, however, I was ignorant till within the last few months. A diamond had been obtained in 1892 embedded in a garnet (pyrope); and in another specimen no less than six diamonds occurred closely associated

[1] See *Geol. Mag.* 1897, p. 448.

[2] Another occurrence of diamond (not very pure) in a meteorite which fell at Novo Urei, Russia, Sept. 22, 1886, is mentioned by Professor Kuntz, *Eighteenth Ann. Report of the U.S. Geol. Survey*, Part V. p. 1195.

with, or indenting, or actually embedded in a fairly large, somewhat irregularly shaped pyrope. The one specimen came from Kimberley; the other from the Newlands Mines, West Griqualand, and it was found by Mr. G. Trubenbach, the managing director in England of the Company, during a visit to South Africa.

In these mines, as in the De Beers Mine,[1] rounded boulders occasionally occur in the diamond-bearing rock—the blue ground (soft or hard, as the case may be). Mr. Trubenbach brought some of these from the former locality to England, and a small diamond was then observed to be exposed on the surface of one of them; the boulder was broken and others were disclosed. One fragment was sent to Sir W. Crookes, to obtain the benefit of his opinion, and he showed it to me. Though I saw it by artificial light, I felt certain that the rock was not any variety of the breccia, but a true eclogite, and expressed that opinion. He most kindly asked me to examine the rock, and obtained from the directors permission for me to cut off as much as I thought necessary for a satisfactory investigation. I am deeply indebted to him for this kindness, and to Mr. Trubenbach for aiding me with other specimens from the mines and responding so willingly to my inquiries. An account of my examination of the whole series was communicated to the Royal Society on 1st June,[2] and the following are the principal results:—

The boulders of eclogite were six in number, but all prior to fracture had been well rounded. Stones of similar shapes might readily be found in the bed of an Alpine torrent after a course of several miles — in other words, I am sure they are water-worn. Three are of one species of eclogite, and three of another; two of the former being known to contain diamonds. That in which this mineral was first discovered is apparently from a quarter to a third of an ellipsoidal boulder, its axial measurements being roughly 4 in. × 3 in. × 2 in. The other specimen, probably about a quarter of the original, measured in the same way about $5\frac{1}{2}$ in. × 5 in. × $3\frac{1}{4}$ in. The outer surface of the former specimen is smooth; the pyropes[3] barely, if at all, projecting. So it has been in the other, but the surface now is slightly corroded. Near the exterior the pyropes, as is often the case, are covered by a dark outer film, thicker than the thumb-nail, but this is hardly perceptible near the centre.

The first-named specimen is comparatively rich in diamonds. Two are visible on the smooth outer surface, a third on one of the fractured faces, and seven on the other, but two of these (partially

[1] The occurrence of boulders in the blue ground of this mine (among them granite and eclogite) was mentioned so long ago as 1893 by A. W. Stelzner, *Sitzungber. u. Abhandl. der Isis*, Dresden, 1893, p. 71.

[2] *Proc. Roy. Soc. London*, 1899.

[3] I follow previous writers in applying that name to the red garnet of this rock and the washings. Its accuracy is confirmed by the fact that magnesia-mica is so abundant in the kelyphite rim.

covered by matrix) possibly may be in reality a twin;[1] five are exposed within a space about three-quarters of an inch square, three of them apparently in linear contact. These diamonds are octahedra (stepped faces), with an excellent lustre, perfectly colourless and clear. They vary in diameter from nearly 0·15 inch to 0·05 inch, and all apparently are embedded in the green part of the rock. In the second specimen only one diamond is visible, and this has been exposed by a slight flaking away from the outer surface. It is in all respects similar to those just mentioned. Each of these boulders, on microscopic examination, is found to be holocrystalline and to consist almost entirely of pyrope and a chrome-diopside. In a thin slice the former mineral is a light tawny red colour, is generally clear, but is much and irregularly cracked, and is occasionally traversed by wavy bands of minute enclosures, one set being branching and root-like, probably cavities, the other filmy, apparently a variety of brown mica, and indicative of incipient decomposition. The "skin" enveloping many of the garnets, especially towards the exterior of the boulder, is mainly composed of a mica of the biotite group, which in the latter case appears to be associated with a chlorite (by passage) and perhaps with a little fibrous hornblende. It is, in fact, a variety of the kelyphite rim, to which attention has often been called, but the radial structure is less marked than usual (so far as my experience goes), the mica flakes showing a tendency to parallelism. The chrome-diopside is the mineral described under that name by Professor Lewis; by others as omphacite or sahlite. In these slices it is a pale, dullish green colour, inclining to olive. The crystals are sometimes partially converted (at the exterior and along cracks) into a mineral, generally in minute matted fibres, but occasionally in grains large enough to show cleavage; these give the extinction of hornblende, and are no doubt the result of secondary change. The unaltered pyroxene shows one strongly marked cleavage (not so close as is usual in diallage), and a second less developed, sometimes almost at right angles to it. The former, as already noticed by Professor Lewis, is parallel to the clinopinacoid, and by measuring some flakes I obtained extinction angles up to quite 35°.[2] This diopside occasionally encloses a small rounded spot, consisting apparently of a serpentinous mineral, much blackened by opacite. I presume that a very few small grains of a ferriferous olivine were originally present, being among the first minerals to separate from the magma. In one of my slices the brown mica attains a larger size (about 0·03 inch in diameter) than at the margin of a garnet (from which it is dissociated), and exhibits a fairly idiomorphic outline (hexagonal prism). In this

[1] The point, of course, could easily be settled, but as it is unimportant I have preferred to leave things as they were.

[2] Professor Lewis obtained an angle of 39°. My measurements were rough, intended only for identification of the mineral.

case it is generally associated with a little calcite, and in one place with a radiating acicular mineral, probably a zeolite; in another the calcite is mixed with a serpentinous mineral. Larger grains of iron oxide appear to be wanting, and I have not observed zircon or spinel, or even rutile or pseudobrookite. Some of them might turn up, as a diamond might do, if more slices were cut,[1] but obviously they are not at all common. The second boulder corresponds so closely in mineral composition with that just described that a separate description is needless. I have also examined a fragment from a third rounded boulder, which when perfect must have been about a foot in diameter. The rock is practically identical with that of the other two boulders, but no diamonds are visible.

Three boulders, apparently without diamonds, represent another variety or species of eclogite. One is a fragment measuring about 7 in. × $4\frac{3}{4}$ in. × $3\frac{1}{2}$ in.; another an unbroken boulder, the girth of which, measured in three directions at right angles, is approximately $20\frac{1}{2}$ in. × $19\frac{1}{2}$ in. × $17\frac{1}{2}$ in.; and the third is a fragment about 3 in. × $2\frac{1}{4}$ in. × 2 in. In all these the outer surface is rather more decomposed than in the three described above, and the same appears true of the rock throughout. It obviously consists of three principal constituents, with a few scattered flakes of a brownish mica. Two of them, the pyrope and the diopside, do not differ from those described above, except that the former is slightly pinker in colour; the third constituent is an altered enstatite. The mica is only moderately pleochroic, resembling phlogopite; a small grain or two of serpentinised olivine (as before) may be present. Apparently the minerals have formed in the following order: (*a*) pyrope, (*b*) diopside, (*c*) mica, (*d*) enstatite. I had slices cut only from the first specimen, as I preferred to leave the second intact, and the third was more decomposed than the others. This rock obviously is closely related to the normal eclogites and to the eulysites—differing from the one in the conspicuous presence of a rhombic pyroxene; from the other in containing that mineral instead of olivine. If a special name be required I should propose Newlandite, but personally should be satisfied with enstatite-eclogite, for I prefer to call attention to relationships rather than to distinctions.

In connection with this rock an interesting specimen may be noticed, which was obtained from the blue ground. It is an irregular fragment between three or four inches long, consisting of crystals of a greyish-green rhombic pyroxene, in which one cleavage is strongly developed, but with a barely metalloidal lustre. They are approximately an inch in diameter, and between them small pyropes are rather irregularly interspersed. As I was reluctant to injure the specimen by cutting off a slice, I removed a few small flakes, which on examination with convergent light proved the mineral to belong to

[1] Five were made from the first boulder, three from the second, two from the third.

the bastite group, and I have no doubt it is the one present in the boulders just mentioned. The specimen accordingly represents a very coarse garnet-bearing bastitite.[1]

One more boulder still remains, though it requires only a passing notice. It is a compact greenish rock with spots of a light-coloured mineral. This proves on examination to be a rather felspathic diabase, with amygdales consisting chiefly of calcite, with chlorite, and a few small groups of zeolite.

These diamantiferous plots in West Griqualand, though on a smaller scale than at the older mines near Kimberley, occur in a similar way, and are formed of a rock practically identical. Those now being worked are three in number, two at least of them being connected by a line of fissure. The rock has now been proved, and galleries have been driven to a depth of over 300 feet, and the boulders above mentioned were found at various levels down to this from nearly 100 feet. A section obtained just south of the middle "pipe" is interesting. Here a gallery was driven between two walls of diabase (? dykes) about four yards apart, and in the interval were four ribs of blue ground, parted by country rock, which is a grey mudstone, sometimes pebbly. The total amount of the two was nearly the same, but the thinnest rib of "blue" (very decomposed) was about an inch in width, while the thickest was rather under four feet. It is strange that the characteristic "breccia" (though rather a finer variety than usual) should have penetrated into so narrow a fissure.[2] The principal areas, however, appear to be "blow-holes," formed in the same way as parasitic cones along a crack on the flank of a volcano.

Thus the diamond has been found to be a constituent of an eclogite, and the parent rock occurs as boulders in the ordinary diamantiferous material (blue ground). I have no hesitation in claiming this coarsely holocrystalline eclogite as an igneous rock, though I am aware that some uncertainty has been expressed on this point; but, as it happens, I have had several opportunities of studying eclogites, not only under the microscope, but also in the field, and am convinced that they are as truly igneous rocks as granites, syenites, or diorites. They are, indeed, rather closely allied with the last named, perhaps also with certain dolerites. The relationship may be expressed by the homely direction: "Put some salt into the magma of an ordinary eclogite and it will crystallise as one of the less acid diorites."

The diamond then is shown to be an accidental constituent of the

[1] Pyroxenites (diallagite, bastitite, etc.) not unfrequently run very coarse, but (so far as I happen to have seen) in rather thin dykes or veins. See *Quart. Jour. Geol. Soc.* vol. lv. (1899), p. 290.

[2] It will be remembered that the Kimberlite of Elliot County, Kentucky, appears to occupy a branching fissure (Lewis, "Genesis and Matrix of the Diamond," p. 64). As this section was obtained in a gallery at a depth of 300 feet it may possibly be misleading, and some of the blocks of mudstone may not be *in situ*, but only great fragments which have fallen into the fissure.

eclogite, as a zircon is of a granite or syenite. It may prove, however, not to be restricted to this one species of rock. I see no reason why it should not also occur in the enstatite-eclogite already described; while the fact that at Kimberley, if not at Newlands, olivine is abundant in the diamantiferous blue ground suggests the possibility that the diamond may also be a constituent of a peridotite. In fact, though I was unable to accept my late friend Professor Carvill Lewis's view that the Kimberlite was an altered peridotite, I fully expected that sooner or later it would be traced back to some very basic rock, probably to a peridotite. The diamond hitherto has only been proved to occur in meteoric iron[1] (Cañon Diablo), and it was made artificially by Professor Moissan by the intervention of that metal. Indeed, on *à priori* grounds I should have expected to find it in a rock less acid than an eclogite. I venture, accordingly, to suggest that the crystallisation of the carbon may possibly have occurred in some very basic magma which was afterwards invaded by one more acid, the eclogite being the result of the mixture. This, however, is a speculation; the fact, I think, cannot be disputed that the diamond has been traced back to an igneous rock (eclogite) and was not formed in the "blue" (Kimberlite).

The boulders described above appear to me truly water-worn; so also are not a few of the smaller fragments. I suspected this some time ago when examining a parcel of "washings" from the De Beers Mines (where also boulders have occurred), but those sent to me from Newlands have placed it beyond doubt; half a small pebble of eclogite is present, while many of the minerals are so well rounded that the darker kinds could only be determined by fracture. But if this be so, if many of the constituents are water-worn, how can the so-called Kimberlite be an altered porphyritic peridotite? We are compelled to regard it as a clastic rock, formed by explosions, which have mingled the shattered constituents of the coarsely crystalline floor with materials derived from the overlying sediments. The comparative abundance of diamonds in the blue ground suggests that they are fairly common in some members at least of the holocrystalline series. Hence it may be possible, by carefully observing the larger minerals found with diamonds, to infer which of them are really its associates. At present, garnet, chrome-diopside, and perhaps iron oxides, can alone be named, but I fully anticipate other pyroxenes and olivine to be added.

Hence, as the blue ground is not an altered peridotite, the name Kimberlite must be removed from the list of that group, and must disappear from science, unless it be retained for this peculiar breccia in which the diamond very commonly is an accidental constituent. The mode of occurrence, structure, and contents of this breccia suggest that it is the result of some kind of volcanic action, but the general absence of scoria makes it probable that the explosions were due to accumulated steam, and were thus of an exceptional character.

[1] The Novo Urei meteorite, however, is said to contain some ferro-magnesian minerals.

Discharges of lava occurred during the Karoo period and probably afterwards (for both the pipes and the surrounding sedimentary rocks are pierced by dykes), while the marked changes in the matrix of the blue ground (what has been one of the great difficulties in determining its real nature) suggest that for a long time it was acted upon by water at a high temperature. Thus the volcanoes did not go beyond the solfataric stage. They occur over a rather extensive district and are fairly numerous—comparable, in fact, with the volcanic necks of Fifeshire.

The diamantiferous boulders obviously have no connection with any existing alluvia. Probably they have come from a conglomerate at the base of the sedimentary series, resting directly on the crystalline floor. Thus far we have no means of determining what the age of the latter may be, but the Dwyka conglomerate of South African geologists—generally assigned to the Permian system — very probably extends beneath the Karoo beds of the diamantiferous region, and may repose on the crystalline floor. On that point, however, we must await further evidence; suffice it to say that the genesis of the diamond in South Africa was not a phenomenon of Mesozoic or later times, but must be yet more ancient.

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The Scope of Natural Selection.

Continued from page 129.

By J. Lionel Tayler.

The Primitive Characteristics of Protoplasm.

In this section I wish to briefly recapitulate a few well-known facts and generalisations, which appear to me to lead to the conclusion that natural selection acting on variations has been the sole means of producing divergence and evolution in the organic world, that protoplasm is never really modifiable, although it may be and has been adapted to a marvellous degree.

In the evolution of organisms certain generalisations have been shown to be in the main true. From the lower to the higher forms organisation tends to grow more complex and also more specialised; this development consists in a qualitative and a quantitative change. In estimating the value of any theory which claims to be able to largely explain the process of evolution this quantitative, as well as the qualitative, change must be kept in mind. If a study of the lower forms of life leads to the conclusion that even here elimination brings about adaptation, and that there is little or no evidence for modification of structure, while when we compare the higher and lower forms we find that the differences are very largely due to an increase in complexity, and that the qualitative difference is merely a further development or accentuation in the more advanced organism of a property which is always present in the less advanced, then it will be evident that the facts are largely in favour of a purely selectionist theory of evolution. That a study of the facts does lead to such a conclusion I shall now endeavour to demonstrate.

In the lowest forms of life we are confronted with a kind of substance (protoplasm) which manifests certain peculiarities which appear at first to sharply distinguish it from inorganic material. Protoplasm from its commencement, as far as we are able to examine it, appears to exist in two more or less distinct forms; these forms are not sharply marked off, but more or less shade into each other, but still are sufficiently clear and distinct to have led apparently to widely

different results. These two forms have developed on their separate lines and have resulted in the most important divisions of organic life, the animal and vegetable kingdoms; and the most marked difference between these two kinds of protoplasm appears to lie in the fact that one has to exist on comparatively complex foods, the other on comparatively simple. Excluding this and other differences, for the moment, from consideration, there remain three peculiarities which distinguish protoplasm from inorganic material:—(1) It is extremely complex in structure; (2) it is remarkably unstable; and (3) it has the power, when placed under suitable conditions, of building up from its environment material similar to or identical with its own.

Lewes, Spencer, and, in a crude unscientific form, many early writers, have noticed certain resemblances between some kinds of dead and living material; these resemblances have steadily multiplied in number, while they have become far more forcible in character during the last forty to fifty years, so that many, perhaps most, scientists are beginning to assume, consciously or unconsciously, that purely physical and chemical causes are or soon will be sufficient to explain the lower and possibly also the higher forms of life.[1] Let us take first the peculiarities of protoplasm which are apparently most allied to chemical and physical phenomena, its extreme instability and complexity. Making a general statement of the characteristics of the chemical elements, it appears that they may be grouped into three more or less ill-defined divisions—those with marked affinities, others with very ill-marked tendencies, and a third intermediary division. Stability is usually associated in chemistry with simple molecular structure; satisfied affinities and compounds are generally stable when they are made up of elements which exhibit strong mutual affinities, combined in such a way that each tendency is more or less completely balanced by others. The more perfectly the elements are brought into contact, the more combination of these elements is accelerated, and, finally, there is an evolution of energy whenever the less stable passes into the more stable.

Chemical instability, on the other hand, is associated with weak affinities, great complexity, and a combination of elements in a form which by readjustment might lead to the formation of simpler and more stable compounds. As there is always an evolution of energy when the less stable passes into the more stable, there is manifestly a storage of potential energy in the unstable forms. The instability and complexity of protoplasm is therefore really not a difference from, but a resemblance to, non-living substances, because its instability and complexity apparently exist under similar, though accentuated, conditions to those cases where the complexity and instability is purely chemical. The distinctive characteristic of living

[1] Verworn in his "General Physiology" gives a fairly complete summary of this position.

as opposed to non-living substances therefore must be found, if it exist at all, in some other property of living matter, and it may possibly lie in the third feature that has been noticed, its power of maintaining a constant mass of unstable substance under conditions which appear to make for disintegration of the substance; and we notice in addition another fact, namely, that while life lasts a continuous series of chemical changes, at some periods less active, at others more, but never entirely ceasing, are always present. Now in this perpetual chemical change some energy is wasted, and passes off into the environment in the form of heat, motion, etc. How does the organism get sufficient extra energy, not merely to maintain but even to frequently increase its complex and unstable substance? The extra energy might obviously be obtained if the organism continually assimilated more complex and unstable food than the ultimate products into which this disintegrated protoplasm broke down. In confirmation of this position it is noteworthy that plant tissues which have reached a much lower point of evolution than animal, and whose tissue change is less active, require less complex food than animals. For synthesis energy is required, and this could be obtained as above from the food material; in addition it would be necessary to have a very slightly conducting substance, such as we have in protoplasm, to prevent energy from being too rapidly dissipated, while every chemical reaction must be extremely rarefied, as any marked evolution of energy would obviously lead to the destruction of the whole organism. The essentials for the physical aspect of protoplasmic life would therefore appear to be, a certain small but constant amount of surplus energy which leads to a very gradual substitution of the less complex into the more complex, and then the gradual breaking down of the more complex protoplasm thus formed, by equally gradual stages, into simpler products than those which had been utilised as food.

It seems, therefore, conceivable, supposing chemical and physical conditions to be favourable, that a purely chemical product might be found which would, if situated in a suitable medium, manifest synthetical and analytical changes without any additional force being required. As further movements somewhat analogous in character to the amoeboid have been shown to be obtainable by chemical and physical conditions alone, as in the experiments of Quincke, Bütschli, and others, and also the various phenomena associated with chemiotaxis, phagocytosis, etc., appear to lead to the same conclusions, it would seem that the earliest forms of life might be accounted for on an entirely physical basis.

In many forms of bacteria, almost all the above conditions are complied with; they do not include any special phenomena of movement, or show any marked reaction to stimuli. There is usually a special temperature at which they grow most perfectly, while below and above this their growth and metabolism tend to cease, and they

will only grow on or in certain media. From a purely chemical standpoint, there is therefore nothing in protoplasmic activity which suggests any new element; that bacteria thrive under certain conditions but not under others, being dependent on their powers of combination and subject to the laws of chemical change, is consequently easily explainable. It may, however, be urged that while it is true that bacteria are sometimes influenced by some slight alterations in their environment, they are often capable of standing great extremes in other directions, and in this respect do not resemble unstable and complex chemical compounds; even this difference, however, does not hold, since there are many chemically complex and unstable compounds which appear relatively stable under certain conditions while they are equally unstable under others. There are, therefore, a set of conditions associated with early primitive life, which, except for the phenomena of fission which Spencer has shown, is, like the other properties of early protoplasm, capable of a physical explanation—are all explainable by the laws of chemical change, osmosis, diffusion, etc.

There are, of course, many fallacies to which one is liable in dealing with such a question; thus the extreme minuteness of the organisms, and our necessarily imperfect knowledge of their life-history and structure make it probable that any present-day explanation will be incomplete.

I only wish to note that this resemblance is likely to be at least partially true. That this apparent closeness of connection between chemical change and bacterial metabolism may appear to future generations less close than it does to us is possible, still the increased knowledge of the higher organisms, the relation of food-supply to bodily exertion, the recent work on digestion, blood-supply, and tissue change, do not lead to a less but a more close chemical analogy; in any case the inference, as far as the present time is concerned, is in favour of a very close connection between the laws of chemistry and physics on the one hand, and the forms of vital activity on the other.

Now, as far as this inference has weight, it must tell against climatic modification in favour of climatic and inter-organismal adaptation, inasmuch as chemical elements have definite affinities, enter into definite combinations in fixed proportions; and as any alteration in a compound, however complex, must proceed along definite lines, it follows that each form or variety of protoplasm, in so far as it is chemical in nature, can only grow and keep active by being fed by *certain* foods which it can make use of, and by being under *certain* conditions more or less favourable to its organisation; and when a sufficient number of these favourable conditions are not present, the surplus energy of the organism must in time run down, and the organism will die because it cannot utilise other conditions.

At the commencement of this article I endeavoured to emphasise the importance of keeping in mind the fundamental distinctions

between accommodations which are the direct result of environmental influence, just as wood becomes altered in its composition by a sufficient amount of heat, and those other forms of accommodations which are the result of the organismal response to its environment, and I pointed out that only in the former set of conditions was it strictly correct to speak of acquired modifications, and further that this somatic responsiveness was not in the least discordant with the principle of selection—it would, in fact, aid selectional development making the process of evolution more rapid. Now just as the chemical analogy tells against climatic modification, and in favour of use-development or organismal response with elimination of the less responsive, so I hope to show in this concluding portion of the paper that every broad generalisation tells against climatic modification, and in favour of organismal response, and I shall endeavour to show that the somatic response becomes increasingly separated off from the germinal, not through any special isolation of the germinal products, but for precisely similar reasons as other organs have become separated, namely, by increasing specialisation and complexity of structure.[1] In this concluding portion, therefore, of the article, I wish to keep these distinctions constantly in view:—(1) The direct climatic response, an external influence or influences producing internal modifications; except in so far as these external forces are destructive, I believe this influence to be negligible. (2) The response of the organism whether it be uni- or multicellular to external conditions and alterations that will ensue through elimination of the less fitted and preservation of the more fitted, internal response to external conditions, and external elimination of the less responsive organisms. (3) The relation, if any, that the somatic response bears to germinal variability.

In considering the chief differences between plants and animals, we find certain more or less constant conditions which lead to the conclusion that protoplasm is not directly modifiable; thus a broad general difference is found between these two great divisions of the living world in the fact that vegetable organisms live on simpler foods than animal. The fact that the fungi and certain insectivorous plants form a partial exception to this rule, only increases the strength of the selectionist position, for, from the fact that the vast majority of the various forms of vegetable life do live on simpler foods than animal, we may infer that the difference in the structure of the protoplasm was not easily overcome, while the constancy of the character of the exceptions now that a change has been produced is almost positive proof that if organisms can be directly modified by climatic action it must be to a very slight degree. The same line of argument applies to the other differences observable between plants and animals. On the assumption that this difference of metabolism

[1] Lloyd Morgan, in his "Animal Life and Intelligence," has put forward a theory of reproductive specialisation to which I am greatly indebted.

is due to a structural difference existing in the protoplasm itself, that the assimilative power of an organism depends not on its environment but upon its structure, and that these structural peculiarities are never modifiable, although they may be adapted through elimination of unfit and less fit, and subsequent reproduction among the surviving favoured organisms, and repetition of this process until a better and better adapted organism is produced, we have an explanation which satisfactorily accounts for both the constancy and the variability of the many forms of plant life.

Again, the constancy of all low forms of life under varying conditions is often remarkable. In view of the fact that these unicellular organisms are not easy to keep under constant observation, that their reproductive power is often enormous, and that it is at present very difficult if not impossible to place them under test conditions to prove whether or no they are capable of being directly modified by changes in temperature, food, etc., it is worthy of note that the few recorded experiments have taken years and not months or weeks to induce any change in the organism, and this suggests elimination rather than direct modification as the main if not sole agent.

The science of bacteriology is surely strong presumptive evidence that no very rapid modification of form and habits is affected by altered conditions in these low forms of life; the constancy of the characters of diseases known to be produced by these forms of micro-organisms, and the fact that the bacteriologist can frequently tell by the form and behaviour of the bacillus, micrococcus, etc., what disease it will induce, and this in spite of the immense capabilities for modification under changed conditions, etc., that its habits afford, are all arguments against direct climatic accommodation.

Another point which appears to me to throw very considerable light on the subject is the behaviour that all organisms, as far as I know, without exception, exhibit towards their environment. Local conditions of light, heat, food-supply, do not appear to modify organisms in a certain definite manner as one would expect were direct climatic accommodation possible; on the contrary, the action of every organism to its environment, from the lowest to the highest, appears to be selective, the response of certain internal activities to outside conditions. Recent observations made on the phagocytes of the blood show that the determination of their movements is partly chemical, that they move away from some and towards other products; their action is selective. Plants living on the same soil do not make use of the same material, and it is perfectly extraordinary what minute quantities of a substance can be utilized if it be needed by the organism. Iodine and its selection from sea-water by some forms of sea-weed is a case in point. Precisely similar results occur in the animal kingdom. The same choice of food is manifested in different

animals choosing different foods, the same blood circulating in the body of one animal yet has different substances extracted from it by different tissues; wherever we look we see life display this selective action towards its environment; if the materials that supply its needs are not present, the organism dies. This constant and universal tendency in living tissue to select out of many substances its own particular foods is not favourable to any theory of direct climatic modification; it does, however, favour the principle of selective adaptation.

The phenomena grouped around reproduction, in so far as it consists in conjugation and sex differentiations, seem to me to be explainable only on the assumption that protoplasm is scarcely, if at all, climatically modifiable. The simplest form of reproduction is that of simple fission; the single celled organism in which it occurs splits into two or more divisions. Spencer has suggested that the reason for this division may be, that unless very exceptional conditions of growth arise, there will be a constant tendency for volume to increase relatively to surface, and consequently that a point would at last be reached when certain portions of the cell would be insufficiently nourished. To decrease bulk and increase surface division would be necessary; such a theory of fission formed on mechanical grounds offers no difficulty to selection or other theories.

But if the relation that bulk bears to surface determines fission, it follows that fission will be favoured, as we have seen, by poor food-supply and by rapid metabolism, while the opposite conditions will favour slow metabolism; under the first set of conditions a small rapidly dividing cell would be favoured, while conditions that favoured slow metabolism would produce a large cell. On any system of climatic inheritance, the structure and needs of the organism would be modified according to the environment, hence one can see no need for conjugation. On any hypothesis that relies mainly or wholly on selection, it is, on the contrary, easy to understand that union of two nearly allied individuals would tend to preserve the stability in so far as they were allied, and would promote variability on the unallied smaller portion; there would be as a result an increased number of possible variations to select from, and those organisms in which conjugation occurred would be more likely to survive under all conditions, as they would always tend to adapt more readily. A certain limited unlikeness in the two cells which entered into combination would be favoured by natural selection, in order to preserve this necessary variability. This unlikeness might be the beginning of sex differentiation. The fact that conjugation occurs at all, may be explained in part by the fact that all living tissue has a certain selective affinity (and in this it presents many analogies to non-living) for what it has need of; conjugation might be merely the satisfaction of an organismic need.

The fact that the male cell is in some cases attracted to the female by chemical products[1] is some confirmation of this view. Conjugation would thus be allied to the phenomena associated with assimilation.

So far, therefore, the evidence appears to be in favour of protoplasm not being at any period directly influenced by climatic conditions. Protoplasm everywhere exhibits a tendency to select its food from its environment, and when it is unable to obtain such food, or is subject to conditions of environment which are unsuitable, it appears not to be rapidly modified, but is apparently eliminated. Protoplasm manifests in its different forms considerable resemblance to the more complex non-living chemical products, and this, so far as the inference is justifiable, points to the conclusion that certain conditions are essential for its development, that different forms of protoplasm require different conditions of environment, and that when any organism is not in sufficient harmony with its surroundings it is unable to live and is therefore eliminated. The constancy of the differences of the early forms of life would seem also to lead to the conclusion that protoplasm is never, or at most with extreme difficulty, *directly* modified by external influences. Lastly, the facts associated with conjugation and sex differentiation are apparently only explainable on a pure or nearly pure selectionist hypothesis.

Turning to another aspect of the facts relating to life, we find that while very considerable specialisation may be developed in unicellular organisms, yet when these organisms multiply they do so with very little alteration of the mother plasm, reproduction consisting in the separation of a portion of this mother substance, this portion, whether small or large, becoming a separate organism.

In multicellular organisms, on the other hand, we see, besides this method of reproduction, another kind, which very early in biological evolution takes precedence over the more primitive method. The younger organism is developed from a structure that is not represented in the adult form, and the younger organism begins to closely resemble the older only after a period of development. In what respect is this latter kind of reproduction superior to the former? In the hydra we have an organism in which these two types co-exist. A new organism is sometimes developed as a simple out-growth of the mother substance, develops a mouth and tentacles, and with this new mode of obtaining nutriment gradually loses its connection with the parent organism and becomes independent. In other cases we find interstitial cells collecting into groups at different parts of the organism, in some of these groups the inner cells becoming slightly altered in shape, and developing thin, ribbon-shaped pieces of protoplasm or tails, by the aid of which they become capable of considerable powers of movement, and thus provided escape from the hydra into its surrounding medium. Other groups of cells undergo a different change, one

[1] Hertwig's work on "The Cell" gives a brief résumé of some of these cases.

cell, again occupying an internal position in the group, enlarges at the expense of the surrounding cells, and when it has attained a certain size ruptures from the capsule which surrounded it, extrudes two nuclear portions of its substance (polar bodies), and if one of the smaller active cells comes into contact, and fuses with it, it will commence a series of cell divisions accompanied by increasing growth, and develop into an adult hydra similar to its parent. This sexual mode of reproduction very rapidly supplants all other forms; it is probable, therefore, that there is some immediate advantage resulting to the organisms which reproduce in this way rather than by budding. The most obvious difference in these two methods is that there is a great reduction of tissue material, much less being required for this mode of development than the other; it is therefore less expensive to the parent organism. Apart from this there is the additional factor that it would be the most suitable for development, if direct climatic accommodation does not take place, owing to its being the best means of obtaining the requisite amount of variability. This reduction must presumably be largely quantitative and not qualitative, since we find that under very dissimilar conditions a complex hydra can be formed, provided portions of both ectoderm and entoderm are preserved.

Now, where this sexual mode of reproduction arises, we have to consider a new set of conditions; we find that each individual appears to go through a stage of development, maturity, and decay, and that during maturity the reproductive power of the whole organism is best developed.

Perhaps one of the most striking facts associated with the higher forms of life is that these three periods of growth, maturity, and decay in the whole organism do not correspond in time to similar periods in the several different parts of the organism in question. This fact appears to be universal in its application; how is it to be explained? Now, as I have already noted, the most marked difference between unicellular and multicellular reproduction consists in the fact that the latter develops chiefly by a quantitative evolution from a cell which is quantitatively undifferentiated, while the former reproduce by splitting off a portion of their structure, so that in most particulars, except size, the parent and the offspring are identical. Now one of the peculiarities of development and growth in one of the higher organisms is just this quantitative development, and we must assume that the morphological element is present, for it is inconceivable that actual differentiation of structure could arise without some structural difference for its starting-point. We are bound therefore to assume two positions as essential to development: (1) Some basis for the differences that are found in individual development which must be of a structural and not a physiological nature, whether we call them gemmules, physiological or morphological units, biophors or stirp; (2) that development consists largely in a reduplication of

parts which at the time of fertilisation are somehow or other qualitatively represented in the fertilised ovum.

In development every organism passes through a series of stages which are more or less proportional to its specialisation and complexity, and the definite stages are passed through in a definite order, the highest specialisations, except where definite atavisitic or degenerative phenomena intervene, always coming at the later periods of development. When decay sets in in the organism we not uncommonly find that this order is reversed, the higher being the first to disappear, just as they were the last to come. In the action of many drugs we see the same tendency; if their action is general, the highest nerve-centres go first, the lowest fail last. Now this sequence in development, since it is so universal, must serve some purpose. The very early stages of segmentation appear to be little else than quantitative in character, but later qualitative differentiation begins to be manifested. The study of life in recent years has shown conclusively what an enormously important part the various products of tissue metabolism exert over life; the toxic and anti-toxic theories in disease, phenomena associated with internal secretion, the influence of vegetable alkaloids on different animal tissues, etc., all go to show that tissue activity is very dependent on its surroundings for its activity. Some facts of embryology lead to the conclusion that some organs have an almost purely developmental significance, and are of little use to the developed organism. We know also that organs vary in their relative importance and size to the whole organism at different periods of its development. How are we to explain the cause of this atrophy of some organs while others are developing, except on the assumption of a chemical food sequence? If we assume that, with a growing specialisation, itself induced by the liberation of metabolic products in the preceding stages, there is a growing specialisation of ferments and other material necessary to a more developed organism, and as a consequence a growing specialisation of all food material, we shall have a theory in accordance with facts, and which can explain many otherwise incomprehensible phenomena. The more specialised the food products circulating in the organism, the less favourable the conditions for the more generalised tissues; hence the progressive development of some tissues, and atrophy of others, would be explainable.

The sequence in development would then be itself explainable, as the higher could only be developed from the lower through this sequence; hence the necessity of recapitulation of the ancestral types in development. Rudiments would on this theory disappear in proportion to the generalised character of the rudiment as compared with organismal specialisation, and this would apply to germinal and somatic development. On this theory the whole organism would continue specialising so long as the morphological elements allowed of further differentiation; when this limit of specialisation was reached the organism

would arrive at maturity, and, so long as each tissue remained proportionately active, health would result, but when this balance failed degeneration and disease would result.

We come now to the concluding question, the relation that germinal development bears to somatic.

As an organism reaches maturity, the phenomena associated with reproduction become manifest; this fact is practically universal, it holds good for multicellular and unicellular organisms alike, and for both the animal and vegetable kingdoms. In unicellular organisms, as we have seen, it is probable that there is a mechanical limit to the size of the cell, beyond which growth as a single cell becomes impossible; this growth limit will not be the same under all conditions, but must ultimately be reached in all forms of single-celled organisms.

In the metaphyta, under suitable conditions, there appears to be a nearly constant tendency to growth at any place where a breach of continuity is formed in a living tissue or tissues; in the lower forms of metazoa removal of a portion of tissue is nearly always followed by growth of the remaining, so that more or less complete repair results; in the higher animals, on the other hand, this local reparative process is much less complete, yet even here some attempt is always present.

The fact that removal of tissue tends to produce activity and growth at the seat of injury suggests that possibly some mechanical limit to growth is one of the causes of cessation of growth.

The inferences so far necessary to determine the relation that somatic development bears to germinal may now be summarised as follows. I have endeavoured to point out that facts do not favour direct climatic modification, and I accept the Neo-Darwinian conclusion and believe that there is very little evidence for the transmission of somatic responses. From a study of facts which have universal applications I have endeavoured to show (1) that growth and reproduction are in some way closely related; (2) that facts justify the inference that an increasingly complex food sequence prepares the way for morphological quantitative specialisation; (3) that some morphological explanation of heredity is necessary to explain the facts. Some such provisional theory as the following would, I believe, explain the facts of heredity, growth, decay, and certain facts which have reference to disease, better than previous theories:—

1. That there is a mechanical nutritional limit of growth for each cell, that this bulk limit varies according to physical conditions and food supply, but is reached sooner or later by all growing cells (Spencer). When this limit is reached, cell division takes place, which may be equal, as in fission, or unequal as in budding, etc.

2. Under conditions which demand variability of the organism, conjugation of similar organisms placed under similar conditions would be favourable for the attainment of this requisite variability. If protoplasm is never directly modified by climatic conditions, then the

best chances of survival and adaptation, either to old or to new conditions, would be through conjugation. Selection would therefore favour conjugation (Weismann).

3. If for some reason, possibly nutritional in origin, fission in an organism had not been quite complete, and the cells instead of separating had remained together, then as each new division reached maturity it would divide and the process of division would continue till interfered with by some outside condition, many different forms of these masses of cells would thus be produced, examples of which may be found in the different forms of sponges. Now, if for any reason a curved single layer of cells was formed, it would go on growing in all directions until it met other cells of the same collective cell colony; a multicellular growth limit would thus be reached. Now, assuming this growth capacity to remain constant, one of three things can happen. With a somewhat irregular hollow sphere of cells, it would be conceivable that: (1) a bending in at one of the weaker points, or (2) a bending out would occur, many cells being involved in this yielding; or (3) each cell might bud off a certain portion independently. Of the first or outward yielding, and the formation of buds, we have many examples occurring in nature, as, for example, bud development in the hydra; of the inward yielding, the passage from the blastoderm to the gastrula stage, through the process of invagination occurring in the development of many animals, affords an example of the second means of satisfying this growth tendency; while in the third case division of the individual cell, and separation from its parent tissue, occurs in the formation of red blood corpuscles in mammals, etc.

4. It is obvious that the general structure of the organism would be least disturbed by each individual cell throwing off buds, and therefore the more specialised the organic structure the less likelihood of those organisms that reproduced by any collective alteration of the the organism surviving. With growing specialisation each tissue will become less and less able to reproduce other than its own specialisation, hence reproduction will occur only when the buds from the requisite differentiations meet; now in the case of the hydra it appears to be only necessary to have representatives of two classes of cells, the ecto- and entoderm, and these thrown-off portions of cell structure would, when the requisite number met, owing to perhaps some stronger growth tendency, tend to push up the cells above them, and as the most likely place for the ectoderm and entoderm units to meet would be *between* these two layers, we should expect development to commence from this position. With increasing differentiation reproductive centres would tend more and more to be localised to one centre. Hence with increasing specialisation there would be progressively less power of local or somatic reproduction.

5. A special kind of organism survives for two reasons: (1)

because it is suited to its environment; (2) because it can reproduce similar organisms in sufficient number to maintain or increase its relative position in its environment. The more perfect the organism the less its chance of elimination, consequently so long as its reproductive power is successfully maintained it is to its advantage if it can reduce to a minimum the loss incurred by the organism in successful reproduction; it will follow, therefore, that the cells which throw off least reproductive material from the adult structure will require less nutriment, and therefore the collective organism will, other conditions equal, survive under competitive conditions. For this reason protoplasmic growth will be reduced as far as possible when beyond the needs of the organism, and the reproductive buds or units from each cell will tend to be reduced both in size and number. For these reasons it would obviously be of advantage if merely the morphological elements were extruded from the different cells,[1] and these when collected in the reproductive centre would form the material for the new individual.

6. As differentiation of reproductive function continued running a parallel course with other specialisations of structure, natural selection continuing to favour the best-formed individual and offspring that environments could allow, two tendencies would become manifest: (1) a tendency to reproductive economy, by which every unnecessary development would be eliminated so as to make reproduction a less and less expensive process to the organism; (2) owing to increased complexity, specialisation, and evolution of structure, reproduction would become a more and more delicate process, and would constantly have to be conducted with increasing care, and the stages of development of the organism would therefore become increasingly prolonged. The development of the individual, and the capacity of that individual when developed for competition with other individuals, would form two partly competing and partly complementary elements of race progress, and the resultant of the two would correspond to the line of progressive adaptation and development. With the increasing length of the period of development differentiation of sex becomes first an advantage and then a necessity.

7. A progressively specialised method of food supply will be required to keep pace with the other specialisations.

In applying these conceptions to the interpretation of phenomena, certain points must be specially emphasized:—

(*a*) Every important specialisation of structure must be represented.

(*b*) As, however, one of the causes of evolution of structure is quantitative complexity, it follows that every quantitative element need not be represented, but only the right

[1] In the extrusion of the polar bodies from the ovum, we may possibly have an instance of what on a smaller scale is universal among multicellular organisms.

proportions preserved between the various qualitative specialisations.

(*c*) Reproduction on this theory commences when full or nearly full development of a structure is reached, when its growth capacity is in excess of its demands; from this it will follow that the reproductive units will be collected in the reproductive organs in the order of their evolution.

(*d*) A progressively specialising food supply would determine the development and the atrophy of the different reproductive units.

(*e*) The later a specialisation was developed either in the history of the species or the individual the less chance of its obtaining a foothold in reproduction, and conversely these must be the first to be eliminated under stress conditions. It will follow from this that the effects of use and disuse, in so far as they are of a somatic nature, will be very little if at all transmitted to the germinal structures, since development, in so far as the major part of the organism is concerned, will be completed early.

The first advantage of a theory like the preceding is that it has no need for the supposition of any isolated germ structure, use-inheritance being largely negatived by specialisation. The relation of germinal to somatic development is on this theory understandable. It would account for recapitulation in development, not on the ground of a tendency in the organism to repeat certain ancestral characters, but simply as the necessary preparatory specialisations out of which the later ones are built.[1] It would divide all anomalies into—(1) those cases of faulty representation due to the missing of some prior stage in development, as in the case of cretins, where the morphological element is there but the means of developing it is not, or where deficiency of the element itself as possibly happens in the case of mongoloid idiots; (2) disproportionate representation (quantitative anomaly), leading to dichotomy, etc.; (3) under rare conditions the reappearance of real ancestral characters.

If therefore the recapitulation theory has a different meaning from that of ancestral repetition, and if most cases of so-called atavism can be explained on the assumption of incomplete development, if it is further borne in mind that given the power of segmentation then all that is chiefly required is a proportionate representation of germs, then the complexity of the germ plasm, although very great, need not be so inconceivably great as that which involves the representation of a large number of ancestral as well as all living characteristics. Normal sexual reproduction would on this theory be the right

[1] In a limited sense, however, these stages would represent the history of the individual ancestral line.

principle for selection to rely upon, since the male and female lines of heredity would be largely in harmony over the earlier stages of development, the tendency to vary being increased towards the later stages, thus the requisite stability and variability would be largely obtained. Finally, this theory involves no very great assumption; it is, when examined, very little more than a series of inferences drawn from peculiarities of life that appear to be nearly or completely universal in application, being dependent solely on the assumptions of mechanical and chemical limits to growth, the latter being no longer an assumption, but an established fact in some instances, on the innate capacity for growth, qualitative and quantitative specialisation, and upon the conclusion that protoplasm is never directly influenced by climatic conditions. The theory of co-incident variability and the non-inheritance of acquired responses would equally accord with this theory as with Weismann's, while it would account for those cases of modifications which have been effected during the early stages of development.

In conclusion, I have endeavoured to show reason for believing that the principle of selection, when rightly viewed, is the only theory which is capable of explaining the various phenomena in their entirety; that the properties existing in the lowest forms of life do afford sufficient material for natural selection to act upon, and therefore, until it can be shown that another theory is in more complete accordance with the facts, that natural selection must be regarded as the dominant factor of evolution.

THE GROTTO,
HAMPTON-ON-THAMES.

Suggestions upon the Origin of the Australian Flora.

By Spencer Moore, B.Sc., F.L.S.

Of all the problems which have engaged the attention of those biologists for whom questions relating to the distribution of life upon our globe have possessed special interest, none has appealed with more fascinating insistence than that one which concerns the stocking of Australia with its animal and vegetable inhabitants. Many are the memoirs wherein this subject is treated either as a whole or in some special and subsidiary aspect. The former method, the method adopted, for instance, with so much brilliancy by Mr. Wallace, is, of course, the more satisfactory one, inasmuch as the same general principles must—due regard being paid to special circumstances in their application to each individual case—have been operative in all departments of both kingdoms of nature. But although only the scantiest reference to zoological problems is made in the following pages, it is believed that the views maintained in them are in no way discordant with the ascertained facts and recognised deductions of zoology: indeed, were this not the case, the task I have set myself would be a hopeless one. But it is otherwise difficult enough, involving, as it does, rejection of views which have received such weighty advocacy, both here and on the Continent, as has raised or almost raised them into the rank of axioms of science.

Before explaining my ideas, however, it will be necessary to dwell for a time upon one theory to which general adhesion has been given, in my opinion, without sufficient warrant. Basing their conclusions to some extent on zoological data, and swayed by the bias imparted by those data, botanists have assumed that the Australian flora is of a lower and less specialised type than that of the northern hemisphere and the tropical regions. It exists to-day, we are told, simply because it has remained isolated from the great land areas of the Old World, and but for this, an exotic flora would have overrun the island-continent as certainly as, without the interposition of the ocean, the Marsupial and Monotrematous fauna would have disappeared before the inroads of higher Mammalia better adapted to the conditions of

existence. It is proposed now to throw the search-light of analysis upon this theory, with the object of ascertaining whether it rests on a real substantive basis or no.

The first point to be dealt with is the idea that species belonging to genera predominantly extra-Australian must necessarily have had their origin outside Australia, whither they have migrated, some inherent superiority possessed by them over forms truly endemic having enabled them to maintain themselves and gain ground in their new home. In this relation the two floras of special concern are the Scandinavian and the Indo-Malayan. "The Scandinavian asserts his privilege of ubiquity," writes Sir Joseph Hooker,[1] and the same botanist tells us he regards "the Indian plants in Australia to be as foreign to it, botanically, as the Scandinavian, and more so than the Antarctic."[2] Mr. Darwin[3] goes so far as to ascribe the "aggressive power" of the Scandinavian flora to the fact of that flora having been differentiated in the most extensive land-area of the globe, where competition has been most severe and long-continued. But the supposed long continuance of this competition traverses well-established geological data, which teach us that the undisputed sway of this flora over Northern Europe and Asia dates only from post-Miocene times; while as regards the nature of the competition, who can possibly say that European plants have been subjected to greater stress than those of the old and new world tropics, of South Africa, or of Australia itself? Mr. Wallace[4] has no doubt about this Scandinavian predominance, though he is neutral as regards Mr. Darwin's explanation of it; and Professor Tate,[5] who has recorded his recent experiences in Central Australia in an ingenious and suggestive memoir, finds warrant for the belief that an exotic vegetation is there gaining the upper hand over the indigenous flora. In the face of such authority, and more could be cited were it necessary,[6] it will, I hope, be believed that the attempt here made to maintain a contrary opinion is undertaken in a spirit of diffidence, and without the slightest desire of asserting a rebellious originality.

It is not to be doubted that during past ages facilities have existed for the transport of northern forms through the tropical highlands into southern countries and *vice versâ*. Whether this migration has been largely favoured by cooling of the tropics during glacial periods, or whether, as is perhaps more plausible, it has been in great part due to transport by ordinary agencies such as the winds, the movements of birds, etc., is not a question we have here to discuss. Under the first supposition it is difficult to understand, as Sir Joseph Hooker has pointed out,[7] how tropical species could have survived, though, as the

1 "Flora of Tasmania," Introd. Essay, p. ciii.
2 *Loc. cit.*
3 "Origin of Species," ed. vi. p. 340.
4 "Island Life," p. 511.
5 "Botany of the Horn Expedition," p. 120.
6 These remarks being of the nature of suggestions merely, I have refrained from quoting bibliography except when that course seemed unavoidable.
7 *Trans. Linn. Soc.* xxii. p. 259.

supposition deals with *secular* changes, that is with conditions entirely outside the limited range of our experience, speculations on the subject cannot be said to be quite conclusive. The fact we have to recognise is that migration has taken place, whatever may have been the agency or agencies whereby it was effected.

Now, the most successful migrants should be herbs, for the seeds of herbaceous annuals falling upon favourable soil will rapidly germinate, and the seedlings will run through their life-history in a season lasting only a few weeks. So, too, free-seeding biennials and perennials will take possession of an unoccupied area, and produce offspring soon ready in their turn to extend the range of the species whenever occasion offers. Far otherwise is it with shrubs and trees, which require several years before they bear seeds. Competition, too, between trees and shrubs will be much keener than between herbs; for each of the former must have a considerable space for the support of its assimilating organs; their area also will be limited by such a condition as depth of soil, and they are liable to destruction by storms. Moreover, unoccupied spaces are left between them, and here herbs can flourish. And when it is remembered that the stepping-stones, as it were, which have been made use of in the transport of plants across the tropics—the mountain-ranges, that is to say — are especially adapted to herbs, many of them living above the regions of trees and shrubs, we see how great an advantage in migration has been enjoyed by herbaceous plants over woody ones.

We come now to the next point, which is, that while in the north part of the northern hemisphere the proportion of herbs to shrubs and trees is so large as to justify our calling this portion of the globe a herbaceous zone, the south part of the southern hemisphere, where it is not occupied by the ocean or by glaciated land, comes for the most part within what I shall term a dendritic zone, meaning by this a zone where woody vegetation predominates over herbaceous. New Zealand, temperate Australia, South Africa, the greater part of extra-tropical South America are all dendritic lands. Given, therefore, opportunities of transport from either hemisphere into the other under conditions similar or approximately similar to those now existing, and herbs being better adapted to transport than woody plants, the probabilities are that the preponderating trend of migration will be from north to south, and this without any inherent superiority in the northern flora due to competition in the largest land-area of the world, or to any other cause whatsoever.

"But," one fancies an objector saying, "consider how large a number of northern species have passed over into the southern hemisphere, and how few and far between, and even then how limited in their range north of the Equator, are the southern types which have succeeded in gaining a foothold in the northern hemisphere." But this statement assumes our possession of more knowledge than is at

our command. Is it so certain that all the species of the Scandinavian flora have originated in the northern hemisphere? Sir Joseph Hooker, it is true, guards himself verbally from this assumption by enumerating certain genera found south as well as north of the tropics as "eminently characteristic" of the northern flora. But the inference remains nevertheless, and we have only to consider the case of the Marsupials and Monotremes—orders "eminently characteristic" of Australia, but which we know upon zoological evidence not to have originated there—we have only to consider this case to see how unjustified is the inference, and how liable we may be, by adopting it, to fall into complete error. And it will be well here to deal, by way of example, with a few genera usually regarded as of northern origin, but which, it is maintained, may have originated in the southern hemisphere. There is *Senecio*, for instance, a genus strongly represented in extra-tropical South America (Philippi enumerates no less than 117 species as members of the Chilian flora alone) and in South Africa, and less strongly in Australia and New Zealand. The general view about such a case as this is that the areas just mentioned are isolated from each other, while each is in complete or almost complete connection with the great northern continent; hence the probability is that they were stocked from the latter. But, given a means whereby the species of *Senecio* could pass from north to south, there is no inherent reason why they might not have migrated in the opposite direction, say, for example, from South Africa by way of Eastern Asia into America on the one hand, and *via* what is now the Indian Archipelago into Australia on the other, and certain affinities between the floras of South Africa and Australia seem to show that some such migration has actually occurred. Again, take *Drosera*, a genus which, from the bias of early association, is usually regarded as having originated in the northern hemisphere, but which, in point of numbers and of differentiation, is far better represented south of the Equator than north of it, and very strongly in Australia itself. Then there is *Veronica*, with 15 Australian and no less than 40 New Zealand species, with 18 species in India, chiefly the Himalayas, about 20 species in North America, and not quite so many in China. Out of a total of some 160 species for the whole world rather more than one-third are natives of Australia or New Zealand or both. *Aster*, too, is a case in point, for though the Australian *Olearia* and the South African *Felicia* have been separated from it, and may still be kept up for convenience sake, in no essential respect do they differ from *Aster*, of which over 200 species are North American, while there are about 50 species of *Felicia* and nearly 70 species of *Olearia* in Australia and 20 in New Zealand. Now *Aster* is a genus eminently characteristic of the nearctic portion of the great northern land-mass, but if it had a northern origin, why is it so rare in Europe, a region where many of its species have become naturalised and are able to maintain themselves? Why may

not the genus have originated in Australia and passed thence *via* Eastern Asia, where it is represented by several species, into North America? Only on the hypothesis that a genus must have arisen in a larger area and that its presence in a smaller area must be due to migration, which is a mere begging of the question, can the possibility of a southern origin for *Aster* be denied. Mention may be made, too, of *Bassia*, in Mueller's sense of the term, that is, as comprising *Chenolea*, *Sclerolaena*, *Anisacantha*, *Threlkeldia*, and part of *Kochia* as understood by Bentham. Of these *Sclerolaena*, *Anisacantha*, and *Threlkeldia* are endemic in Australia, and the two species of *Kochia*, referred to *Bassia* by Mueller, are also endemic there, *Chenolea* alone being extra-Australian with nearly one-third of its species restricted to the island-continent. Yet *Bassia* is held by Professor Tate to be a genus exotic to Australia! So, too, *Kochia* proper has 19 Australian species, all endemic, leaving only 13 to be shared between South Europe, temperate Asia, North and South Africa, India, and North-West America; and when we remember that several peculiar genera allied to *Kochia* are exclusively Australian, is there anything extravagant in the opinion that probabilities point to this genus as having originated in Australia? And what shall we say of *Atriplex*, of which many species are Australian, and some of them extraordinarily abundant in individuals? The evidence for a southern origin of such genera as *Ranunculus* and *Clematis*, *Myosurus* and *Samolus* is not so strong; but when we come to aquatics, such as *Callitriche* and *Ceratophyllum* and *Potamogeton*, all very extensively distributed, I do not see upon what grounds the possibility of a southern origin for some of them can be scouted, and it must not be forgotten that *Myriophyllum* belongs to an order reaching its maximum of species in Australia. Then take the Grasses, an order very abundant in both hemispheres. Why may not such genera as *Deyeuxia*, *Hierochloa*, *Stipa*, and *Eragrostis*, to mention a few only, have originated in some southern land or lands, and migrated thence to their present homes in the north?

These are merely a few cases mentioned by way of example: by no means do they exhaust the list of genera for the southern origin of which there is at least some probability. But it may be objected that most of the genera cited above are not found in antarctic lands, and how, it will be asked, is their absence explained if they had a southern origin? I reply that, for all we know to the contrary, antarctic lands may, at some former time, have supported many supposed northern genera now not found there. This traverses Mr. Darwin's opinion when he says:[1] "I am inclined to look in the southern as in the northern hemisphere to a former and warmer period, before the commencement of the last glacial period, when the antarctic lands, now covered with ice, supported a highly peculiar and isolated flora." But with all deference to Mr. Darwin, why should the pre-glacial antarctic

[1] "Origin of Species," 6th ed. p. 341.

flora necessarily have been peculiar and isolated? If there is one point on which students of biological geography are agreed it is this, that the antarctic continent must formerly have extended considerably farther north than it does now, an extension which permitted the migration of certain animal forms from South America to New Zealand, and must equally have allowed the southward migration of South American and New Zealand plants. This stocking of the antarctic continent may have occurred comparatively early in Tertiary times, and so long as glaciation did not supervene, a large and by no means peculiar or isolated flora may have flourished in the antarctic continent. But now, communication with lands lying to the north being cut off, if a glacial period occurred, the result in the southern hemisphere would be very different from one in the northern, for while in the latter there would be nothing to hinder the southward migration of plants, their escape from the antarctic continent would be cut off by the ocean, and since all antarctic lands must have been covered with an ice-cap during a glacial period, all, or almost all, but the lowliest organisms must necessarily have perished. Obviously the nature of the flora of the antarctic continent previous to the last glacial period must have depended upon the occurrence or no of a glacial period or of glacial periods intercalated between the last of such periods and the stocking of the continent when it was in connection or close relation with lands to the north. If no such period intervened, then the flora must have consisted of a mixture of South American, New Zealand, and possibly to some extent of Australian types, or of descendants from such, together with endemic genera, of which many, for all that we know, may have been identical with genera characteristic of northern lands. But if a glacial period was intercalated, and that after the connecting lands to the northward had disappeared beneath the waves, then the flora of the antarctic continent during the subsequent warm period must have been closely similar to that of other antarctic lands, since it would have been derived from the same source or sources; while if the connection with lands to the north was still open at the commencement of the intercalated glacial period or periods, the antarctic flora would have migrated northward, and, the connection being still maintained, would have advanced southward on the return of warmer conditions, so that it would have borne approximately the same *facies* after as before the glaciation of the continent. If this reasoning be sound, therefore, in no event does it seem likely that the antarctic flora could have been in any special sense isolated and highly peculiar.

As an instance of the way in which the brief—if the term may be allowed without offence—for the predominance of the northern flora has been handled, I shall cite the assumption that glaciation first affected the northern hemisphere. Let us hear Mr. Darwin. After alluding to the southward migration of species when glacial conditions obtained

in the north, "then," he says, "in the regular course of events the southern hemisphere would in its turn be subject to a severe glacial period, with the northern hemisphere rendered warmer; and then the southern temperate forms would invade the equatorial lowlands. The northern forms which had before been left on the mountains would now descend and mingle with the southern forms. These latter, when the warmth returned, would return to their former homes, leaving some few species on the mountains, and carrying southward with them some of the north temperate forms which had descended from their mountain fastnesses. Thus we should have some few species identically the same in the northern and southern temperate zones and on the mountains of the intermediate tropical regions."[1] Now we have as much right to assume that glaciation first affected the southern hemisphere; and a clear idea of the result will be gained if the reader will substitute "south" for "north," and *vice versâ* in the above admirable quotation. Yet what a different idea of the trend of migration it gives us!

But my imaginary opponent now proposes to crush me with an argument he has carefully held in reserve. "Consider," he exclaims, "the evidence furnished by introduced plants. Wherever man settles, his footsteps are dogged by Scandinavian species, which rapidly establish themselves in their new home and at the expense of the indigenous vegetation; how could this happen unless there is some potency inherent in northern forms over and above that possessed by the southern flora?" While admitting that a considerable number of northern plants have become naturalised in southern lands, it must not be forgotten that some, though a far smaller number, of southern species have gained a foothold north of the equator. But in order to estimate properly the value of this preponderant naturalisation of northern forms, we must not be contented, although even Mr. Darwin seems to have been contented, with merely drawing up lists of the colonists of either hemisphere; before ascribing any aggressive power to the northern flora, we must ascertain that no other explanation of the facts is possible. And firstly, we note, and it is a matter of great importance, that almost all the plants naturalised in southern lands are herbaceous. We may take as an example Sir Joseph Hooker's list of introductions into New Zealand.[2] It amounts to 170 species, of which fully half are annuals, thirteen are biennials, and over fifty of the remainder, although perennial, are herbaceous. Now what has happened in New Zealand since the first batch of colonists landed on its shores? The densely clothed forest-lands have been cleared to make room for the herbaceous vegetation on which man depends for his sustenance; in other words, a dendritic zone has been artificially converted into a herbaceous one. And not this only, but the seeds

[1] "Origin of Species," 6th ed. p. 339.

[2] "Handbook New Zealand Flora," p. 757.

of these economic plants have been introduced from the north, and at the same time the seeds of other plants accustomed from time immemorial to flourish in association with them, as well as the seeds of species which have been allowed, for the sake of old recollections, to obtain a foothold in the new homes of the race. We have seen how advantageous it is for a migrating species to be herbaceous, and a still greater advantage should obtain where migration has been so effectually assisted by human effort. Then again, a point we ought to have information about, for it has material bearing on the case, is whether the indigenous herbaceous vegetation has benefited by the introduced changes.[1]

But the case becomes still stronger when we take Australia into consideration. The fierce droughts experienced by so large a part of that country have brought about the survival of a vegetation to a very large extent xerophilous. Now there is one peculiar feature about all desert countries except the very dryest, a feature necessarily tending to favour the spread of any herbaceous vegetation of which the seeds may chance to be introduced into them, namely, that at least during some part of the year there are always places where water is apt to collect, and where the ground will remain moist during the short time while the life-history of a herb is being enacted.[2] This is simply what one sees in the interior of Western Australia. For a period long enough to ensure the maturation of their seeds, introduced plants enjoy, in normal seasons, conditions precisely similar to those obtaining in their native habitats. But no sooner does the sun gain in power, and the ground become dry and warm, than these herbs completely disappear; they show, in fact, none of that capacity for adapting themselves to their altered surroundings which we should expect members of an "aggressive" flora to possess. This is, however, not the only advantage "Scandinavian" species enjoy when introduced into a country with a dry climate such as Australia. If one or more seasons of drought supervene, what happens? Considerable though varying power of latency is possessed by the great majority of seeds, and under these circumstances the introduced herb is in precisely the same position as the indigenous, both having to await a favourable season in order that their seeds may germinate. Contrast this now with the fate awaiting seeds of dry southern climates introduced into a country with a climate like ours. A short spell of warmth sets in,

[1] Authoritative information on this subject has recently come to hand (*vide* T. Kirk, Presidential Address to the Wellington Philosophical Society, 1895; abstracted in *Journ. of Botany*, 1896, p. 338). From this it is clear that in some cases indigenous species have benefited by changes due to human agency.

[2] The conditions in Australia are specially favourable to the introduction of cold temperate herbs, inasmuch as it is only when the temperature is low, that is, when the conditions approximate to those of the summer of Northern Europe, that the ground remains moist for any length of time. Then is the only chance for herbaceous vegetation whether endemic or introduced.

and under its influence the seeds germinate; hereupon the temperature suddenly falls, and the young and tender seedlings are exposed, at a critical period in their career, to entirely new and unfavourable conditions, and they perish accordingly. It is therefore no matter for wonder, and still less for drawing conclusions as to "aggressive power" and "superiority" of the northern species, if introductions from the northern hemisphere are enabled to exist and multiply in the southern, while an embargo is placed upon southern species in Northern and Central Europe. Moreover, that this is the real reason why southern species are not domiciled with us seems clear when it is remembered how, in northern countries where the conditions are approximately similar to those obtaining in the southern hemisphere, southern introductions are able to maintain themselves. One may cite, for example, the Western Mediterranean seaboard and the coast of Portugal, where a fair number of southern species—most of them, it is true, South African, from greater facility of intercourse—have succeeded in establishing themselves, and apparently at some expense to the indigenous flora.

There is one country north of the equator where Australian species readily become naturalised. Botanists who hold fast by the theory that the Australian flora is a mere geographical survival have been puzzled—as assuredly they ought to be puzzled—by the headway that species from Australia make when introduced into Southern India; nor does Mr. Wallace's solution of the problem, ingenious though it be, at all relieve matters. Mr. Wallace cheerily avers that this fact is quite in harmony with the presumed predominance of northern forms. "For," he says, "not only is the climate favourable, but the entire Indian peninsula has existed for untold ages as an island, and thus possesses the insular characteristics of a comparatively poor and less developed flora and fauna as compared with the truly continental Malayan and Himalayan regions. Thus Australian plants can compete with a fair chance of success."[1] But what evidence is there for Mr. Wallace's idea? We venture to maintain, on the contrary, that the Indian flora is, in all essentials, a continental one, and, moreover, the "untold ages" Mr. Wallace alludes to are scarcely in point, for what we want is evidence as to the continued insularity, in a botanical sense, of a region which, for many thousands of years at least, has ceased to be an island. But why travel so far in search of an explanation when one is ready to hand? Why not admit that Australian species flourish in the Neilgherries simply because the present climate of that district is suitable to them? And why not go a step further, and allow that if a land connection existed between Australia and South India, and the intervening country enjoyed a climate like that of Australia, a considerable number of

[1] "Island Life," p. 496, note. The fact there cited was communicated to Mr. Wallace by Sir Joseph Hooker.

Australian species, or of descendants from such, would to-day form part of the Indian flora? But if this be admitted, and it is only a logical deduction from the facts, the theory of the predominance of northern forms collapses, and the restricted area occupied by Australian species must no longer be viewed as depending upon some inherent inferiority to northern forms, but simply upon fortuitous geographical conditions.[1]

But we are told that the Australian flora stands less high in the scale and is less specialised than are the floras of northern climates, and if this be true, the point I am trying to argue must at once be given up. But is it true? In what respect, it may be asked, is the flora of Australia less highly specialised? Are not most of the great natural orders strong constituents of it? Trees, some of them of gigantic size, shrubs, undershrubs and herbs, parasites and saprophytes, climbing and carnivorous species, flowers adapted to profit by the visits of insects, and sometimes provided with a complex mechanism to ensure such profit, all these are met with in Australia. In addition, we have wonderful adaptations to a dry climate, and in this respect, taking into account the variety of ways in which the destructive effects of a scorching sun and parched soil are guarded against, the Australian flora is without a parallel the world over. And if these be not evidences of high specialisation, it is difficult to know where one must look for such. In one respect, and in one only, is any inferiority shown, namely, in the comparatively small number of seeds produced. But this does not apply to the herbs, and as for the woody species, it is absolutely essential that the ripening seeds be safeguarded against drought, and the laying on of thick tissues to this end may well be effected at some cost as regards fecundity.

But Mr. Wallace himself gives us an instance where land adjoining the, according to him, previously isolated home of the Australian flora has been stocked to a considerable extent with Australian forms. As I shall have something to say hereafter about this supposition, I will now merely assume its truth for argument's sake. Mr. Wallace,[2] then, supposes the greater part of Northern Australia, previously submerged beneath the ocean, to have become dry land in the middle or latter part of the tertiary period, and the area so exposed to have been colonised partly by Indo-Malayan forms from the north, partly by Australian forms from the south. Now, assuming with Mr. Wallace that the species with Indo-Malayan facies in Northern Australia were emigrants from the north, their considerable numbers prove that there could have been but slight, if any, embargo upon migration from the north when

[1] Since this passage was written, Mr. C. B. Clarke has informed me, upon his personal knowledge of the Neilgherries, that the success of Australian species there has been much exaggerated. In spite of this, I prefer to leave the paragraph as it stands, for it shows, at any rate, to what lengths an upholder of the "northern predominance" theory may be inclined to go when in search of an argument to meet alleged facts hostile to the theory.

[2] "Island Life," p. 493.

Northern Australia was stocked. Why, then, if Australian forms are less highly differentiated and less capable of adaptation than Indo-Malayan, do we find them holding their own to-day side by side with the more favoured northern migrants? Assuredly this is precisely what we ought not to expect if the theory of northern predominance be sound. We ought rather to expect that those migrants from the south which happened to penetrate into the newly raised area would have been rapidly overcome by their better adapted competitors; and the fact that they have not been so overcome should suffice to convince us that, supposing Mr. Wallace's view of the stocking of Northern Australia to be correct, Australian species can compete not unsuccessfully with Indo-Malayan ones in the struggle for existence on a fresh area. In short, what Mr. Wallace supposes to have actually happened in Northern Australia is exactly what I have just now surmised might have happened in India, but for the wide stretch of intervening sea which has prevented Australian forms from entering the Indian peninsula.

And when we come to consider the extinctions that have taken place in the Australian flora since earlier tertiary times, we find ourselves face to face with a number of facts which contradict *in toto* the doctrine of northern predominance. The only way of escaping from these facts is to deny the soundness of the conclusions upon which they are based, that is, to throw doubt upon the determinations of the palaeontologists. This is the position taken up by Professor Drude,[1] who not only denies that a flora in many respects more northern than the present flora formerly flourished in Australia, but also questions the former presence in the European flora of many species belonging to orders now characteristic of Australia. Professor Drude cites as an example the genus *Quercus*, which has a wide distribution in space, and contains species showing much adaptability to diverse conditions, facts rendering it difficult to understand how such a genus could disappear from any large area it formerly occupied. This instance, however, is not a very happy one, for *Quercus* is now known to flourish in New Guinea, and it may still be found living in Australia when the northern part of the island-continent has been more thoroughly examined. Moreover, we are only imperfectly informed as to why species become extinct. Why, for example, should so few Brachiopods now tenant our seas? Why is it that the great group of the Ammonitidae, so abundant in Mesozoic times, is represented to-day by but one solitary survivor, or, as some may say, by none? What reason can be given for the extinction of the numerous mammals characteristic of earlier tertiary times? The general principle underlying extinction is, of course, a mere commonplace to-day: it is the application of it to individual instances that is obscure; so much so indeed that, in spite of Mr. Darwin's injunction to a contrary view, I do hold, with all due

[1] "Handbuch der Pflanzengeographie," s. 450.

deference, that a fact such as the survival of *Lingula* through countless ages, while multitudes of closely related and equally effective forms have long been extinct, is not devoid of the element of mystery. Such a consideration as that adduced by Professor Drude seems wholly insufficient to outweigh the life-labours of men like Unger and Goeppert, Heer, Ettingshausen, and others. True, their determinations may sometimes be open to objection; but in such a case as this there seems no alternative but to accept, as correct in the main, the conclusions unanimously recorded by specialists in this branch of the science. When, therefore, one finds in the Australian tertiary flora such characteristically northern genera as *Myrica, Betula, Alnus, Quercus, Salix, Fagus, Laurus, Magnolia*, all of which, with the exception of *Fagus*, now scantily represented on the south-eastern highlands, and possibly of *Quercus* as mentioned above, have vanished like the fantasies of a dream, one cannot repress a feeling of wonder that such a phrase as "the Scandinavian privilege of ubiquity" should ever have been called into use. Most of the above genera, if present distribution is to be relied on, and present distribution is the main support of the northern predominance theory, have had their origin in the most extensive land area of the globe, where, according to Mr. Darwin, competition has been most severe and long-continued, and moreover they are still important elements in the northern flora. On the current hypothesis these favoured forms should have entirely or partially eliminated their competitors, instead of which they have themselves gone to the wall. But besides this we are not entitled to assume that Australia was inhabited in earlier tertiary times by no other "northern" genera than have already been found in tertiary deposits there. It is also inconceivable that herbaceous vegetation did not then exist side by side with the shrubs and trees whose harder parts have ensured their preservation in the fossil condition. But before we are in a position to state what this herbaceous vegetation really was, Australian tertiary deposits must be examined in the way in which Mr. Clement Reid is now examining our tertiary beds with such interesting results, for the ordinary organs of herbs are of too fragile and evanescent a nature to allow of their preservation, and recourse must be had to the evidence yielded by fruits, and especially by seeds, involving a tedious operation indeed, but one which must be undertaken before we can feel ourselves on safe ground. Meanwhile we cannot close our eyes to the possibility that a fair number of herbaceous species belonging to "northern" genera may have become extinct in Australia since the time when the "primitive tertiary flora" flourished there.

And while we recognise how favourable to the northern flora are the geographical and climatal conditions of Northern Europe at the present time, it should not be forgotten that such was not always the case. In Miocene times, for instance, when Greenland enjoyed a climate similar to that of Southern Europe to-day, where was the

"Scandinavian" flora? A considerable portion of it must have been in existence then, and it is difficult to conceive how the ancestors of so large and important an element in the earth's vegetation could have found sufficient room in the few extreme northern lands then suitable to them. But during Eocene and Miocene times a large part of the antarctic continent must have had a climate suitable to the support of "Scandinavian" forms; and if we can suppose, and there seems little difficulty in the supposition, warranted as it is by facts of distribution, that the antarctic continent was then continuous with South America, and had outlying lands permitting of interchange with South Africa and Australia, a portion, and no inconsiderable portion, of the flora now considered to be of northern origin may well have taken its rise in these southern lands. It was probably during the Pliocene period that the Scandinavian flora first became important in Northern Europe. Pliocene times must have been highly favourable to the diffusion of herbs which flourish best in colder temperate climates, for not only did cold conditions then prevail, but there were ready for colonisation large areas raised during the mountain-making Eocene and Miocene periods. It is conceivable, therefore, that much interchange between northern and southern lands may have taken place during this period.

But it may perhaps be that the Pliocene age is too recent for such a relation as has been sketched to have existed between the antarctic continent and lands lying to the north of it, though the recent discovery in South America of a carnivorous Marsupial allied to *Thylacinus* suggests that such a relation existed during later tertiary times. Yet the point to be remembered is that large areas in the south have enjoyed a climate eminently suitable to the evolution of forms best fitted to flourish in the colder temperate zones, and, moreover, that during long periods the larger extent of such areas has been in the south. The problem, too, how southern forms could have reached the north is no greater than the problem how northern forms could have penetrated into antarctic lands. All we know is that a genus could have had its origin in but one area, and that, as regards temperate forms, there is much generic resemblance between the northern flora and the southern; but there is no justification for the view that all the genera common to both had their origin in the north and none of them in the south.

It is also necessary to receive with grave doubt any conclusion relative to the inherent superiority of certain floras as a whole over others, and this although several species of supposed northern origin are capable of ready acclimatisation in foreign lands, and can sometimes flourish at the expense of endemic forms; for in every flora there are species more widely diffused and with greater powers of adaptation than others. Has anybody ever argued, from the rapid spread of *Anacharis alsinastrum* in our streams a few years back, from the way

in which, for instance, *Galinsoga parviflora*, and species of *Aster* are enabled to maintain themselves in Europe, any inherent superiority of the American flora over the European? Yet argument of this kind we find constantly applied to the flora of Australia. Nor is present distribution an infallible index to the place of origin of a genus or species. To take two instances showing the general trend of argument on this subject as bearing on the flora of Australia: *Helichysum* and *Helipterum*, although well represented in Australia, are found also in other countries; consequently, it is said, they are exotic genera which have at some time migrated into Australia. Why may not they, as well as other genera, be descendants from the constituents of the "primitive tertiary flora"? Professor Tate partially adopts this view, for he remarks, *àpropos* of certain genera found fossil in tertiary deposits, such as *Ficus, Loranthus, Pittosporum, Santalum,* and *Cassia*—that most of these genera, "when viewed by their present geographical distribution, are considered Oriental; but in regard to their distribution in time they belong to a cosmopolitan flora, which originated in late Cretaceous times in Europe, North America, and Australia; hence their modern representatives may actually be descendants of primitive Australian species, and not modified immigrant forms."[1] But though he makes this highly important admission, in practice he adopts the conventional view, for we find him distinguishing "immigrant" genera and species from "endemic" ones with confidence as serene as though he had himself been privileged to watch, through long ages, all the various steps in the stocking of Australia. Of course the view I am advocating cuts both ways. The Cambodian *Centrolepis*, for instance, may possibly be the sole Indo-Malayan survivor of a genus which had its origin in the Indo-Malayan region, and migrated thence into Australia. So too *Patersonia* may be of Indo-Malayan origin: even *Casuarina equisetifolia* may be, for all we know, the original species from which its Australian congeners have been derived. Not until all later secondary and tertiary deposits have been thoroughly ransacked, and their respective relations in time established beyond dispute, will it be possible to fix upon that part of the earth where a genus or a species first made its appearance. Until this is accomplished our conclusions can rest on nothing more satisfactory than inferences from present distribution, which, unless they be applied with the utmost caution, may lead us far from the truth.

The most recent and, as having been deduced with full knowledge of modern geological discoveries and after personal inspection of part of the country, the most authoritative conclusions relative to the origin of the Australian flora are those of Professor Tate.[2] The Darling

[1] "Botany of the Horn Expedition," p. 131.

[2] Professor Tate's three memoirs, *The Influence of Physiographic Changes in the Distribution of Life in Australia;* Australia's Association for the Advancement of Science (1887); Inaugural Address, in the Association's volume for 1893, and the "Botany of the Horn Expedition" (1896), are most interesting contributions to the subject under notice.

range, he tells us, which is of granite, is capped by conglomerates doubtfully referred by Mr. F. T. Gregory to the Devonian age, but, perhaps, as suggested by Mr. Etheridge, really Mesozoic. Since Upper Devonian times there have always been land surfaces, at any rate in Eastern Australia, where there was partial interruption to absolute continuity during deposition of the Carboniferous rocks. The country presented the aspect of a vast archipelago while the extensive marine cretaceous beds occupying the low-level tracts of the interior were being deposited ; and not until the close of the Cretaceous period was the continent formed. These marine beds—the so-called Rolling Downs formation, of Lower Cretaceous age—were laid down in a comparatively narrow sea connecting the Gulf of Carpentaria with the Great Australian Bight, and there is no evidence for the existence of interoceanic connection since that age, that is to say for the tertiary sea of Professor Duncan and Mr. Wallace. Following close upon the end of the Cretaceous epoch was another submergence during deposition of the older tertiary strata ; but this did not involve so large an area, as these marine tertiary beds are not found more than fifty miles inland except round the Great Australian Bight and in the Murray Desert. After this, by unequal movements of depression, Central Australia became a lacustrine area, for the low-level deposits of this region are of lacustrine origin as their remains prove. Lacustrine conditions continued into Pliocene times, unless the formation known as the desert sandstone, which is of Pliocene age, be eolian, as Mr. Tenison-Woods conceives. The extinct rivers, the circumscribed lacustrine basins marked by their coincident sand-beaches, and the remains of large herbivores prove the climate of Central Australia to have been, up till comparatively recent times, much moister than it is to-day. The subsequent history of the district has been one of gradually increasing desiccation.

(*To be continued.*)

FRESH FACTS.

MICROSCOPIC VIVISECTION. EUGÈNE PENARD. "Sur les mouvements autonomes des pseudopodes," *Arch. Sci. Phys. Nat.* vii. 1899, pp. 434-445. Mr. Penard has made numerous experiments with excised pseudopodia of *Difflugia lebes*, which go to show that detached (non-nucleated) fragments behave for a time as if they formed a complete organism. During their ephemeral life they exhibit movements; they are attracted by plasmas identical with their own, and repelled by those which are unlike.

A WONDERFUL HOUSE. H. LOHMANN. "Das Gehäuse der Appendicularien nach seiner Bildungsweise, seinem Bau und seiner Function," *Zool. Anzeig.* xxii. 1899, pp. 206-214, 4 figs. Dr. Lohmann studied at Messina the history of the house of *Oikopleura*. The foundations are laid in 3 to 4 hours by the energetic secretory activity of special oikoplast cells which form the component membranes and fibrils. The house once begun is quickly finished, and has not been more than a few hours in use before another begins to be built. But what is its use? The answer to this is perhaps the chief interest of this paper, for Lohmann finds that it is justified in three ways. It forms an effective trap for food particles; it serves as a locomotor organ; and it protects the inmate, who can "blitzschnell" leave its encasement and escape with its life.

NOTOCHORDAL CANAL IN MAN. A. C. F. ETERNOD. "Il y a un canal notochordal dans l'embryon humain," *Anat. Anzeig.* xvi. 1899, pp. 131-143, 17 figs. The author has satisfied himself that there is in the very early human embryo a distinct trace of a notochordal or archenteric canal which does not differ in its essential features from that known in other mammals.

HIBERNATING SWALLOWS ONCE MORE. ALAN OWSTON. "Swallows in Mid-Winter," *Annot. Zool. Japon.* iii. 1899, p. 29. In a letter to our Japanese contemporary, Mr. Alan Owston of Yokohama notes that on the 16th of December 1896 he saw a number of swifts (*Cypselus pacificus*) flying about, and that on the 1st of January 1898 he observed a couple of swallows (*Hirundo rustica gutturalis*) catching flies on the beach. "Is it possible that some swifts and swallows remain here throughout the whole winter, and if so do they hybernate in caves like bats?"

WHEN A SNAIL LEAVES ITS SHELL. R. WELCH. "Helices abandoning their Shells," *Journ. of Conchology*, ix. July 1899, p. 217. We had thought that a snail would leave its shell when the Greek Kalends came round, or a canny Scot committed himself to a definite opinion on the weather, but we were wrong again. For there have been repeated stories of late in circulation about snails wandering about in indecent nudity. The *fama* arose in regard to *Limnaea peregra*, but it seems that the more sedate *Helix pisana* and *Helix lactea* have gone in for similar frolics. They were well fed, Mr. Welch assures us, and not handled in any way. This is a "curiosity" which some one will surely soon convert into an interesting fact by telling us the reason why. Is it an atavism before death—a return to ancestral nudity?

FACTS OF INHERITANCE. WILLIAM BATESON and Miss D. F. M. PERTZ. "Notes on the inheritance of variation in the corolla of *Veronica buxbaumii*," *Proc. Cambridge Phil. Soc.* x. 1899, pp. 78-92, 1 pl. Abnormal flowers are of common occurrence in this species, and certain symmetrical forms of variation are especially frequent. Flowers taken at random on heavy clay arable land near Cambridge showed about 6 per cent with 3 petals, and about 1 per cent with two petals, and so on. The experiments described in this paper were undertaken to test whether there is any difference between offspring raised from abnormal flowers, and the offspring of normal flowers borne by the same plant. The evidence, though scanty, goes on the whole to show that there is, at all events in the case investigated, no well-marked difference between the offspring of normal and abnormal flowers.

A PATHOLOGICAL PIGEON. MICHAEL F. GUYER. "Ovarian structure in an abnormal pigeon," *Science*, ix. 1899, pp. 876-877. In a bird which was a hybrid between a Vienna white (*Columba alba*) and a common ring-dove (*Turtur risorius*), the ovary showed a large number of double eggs, that is, two or more eggs within a common follicle. Most of the larger eggs showed vacuoles appearing in connection with the substance of the sphere or yolk-nucleus ; the nuclei in many cases seemed degenerating ; mitotic division of the nucleus was never observed ; many of the eggs, especially the larger ones, were undergoing absorption by means of phagocytes which were the transformed follicle cells. The doubling of the eggs seemed to be due in most of the smaller ones to division of the primordial egg cell and in the larger ones to fusion of contiguous cells. It is not determined that such abnormalities are connected with hybridisation.

SEX IN BEETLES. GILBERT J. ARROW. "On sexual dimorphism in beetles of the family Rutelidae," *Trans. Entomol. Soc. London*, 1899, pp. 255-269. The recorded examples of sexual dimorphism among beetles, other than those which consist in differences of development of various parts, such as the legs, antennae, or mandibles, are at present very few ; but this is partly due to the mistake of referring males and females to separate species. In the heterogeneous assemblage slumped in the genus *Anomala* there is colour dimorphism in species from all parts of the world. The distinction consists not in any fundamental difference, but in the degree of development of the colouring matter, the male (except in two exceptional Mexican species) exhibiting a greater exuberance than the female, or the superposition of a darker hue. In *Anomala imperialis*, discussed in this paper, there is another apparent exception, the colours of the two sexes appearing to be unrelated. But experiment shows that the metallic purple colour characteristic of the male of this species is transformed by exposure to sunlight into a green like that of the female, so that here also the male form is obtained by an addition to that characteristic of the female.

THE AGE OF THE MANX SLATES. H. BOLTON. "The Palaeontology of the Manx Slates of the Isle of Man," *Manchester Memoirs*, xliii. May 4, 1899, No. 1, pp. 15, 1 pl. In this paper (also issued as "Notes from the Manchester Museum, No. 5") are described specimens of *Dictyonema sociale* and *Dendrograptus flexuosus*, found by the writer in small splintery masses of these slates. These indicate that "the stratigraphical position of the slates will be found ultimately to be either amongst the uppermost beds of the Cambrian system, or in the Arenig Series." This conclusion does not conflict with the evidence of the worm castings referred to *Palaeochorda* and *Chondritis*, or the doubtful *Asaphus* also discovered by Mr. Bolton, or the yet more doubtful *Lingulella*, figured by E. W. Binney in 1877. The author is to be congratulated on the light, little though it be, that he has been able to throw on this particularly obscure problem.

SEXUAL DIMORPHISM IN JURASSIC NAUTILI. G. C. CRICK. "Description of new or imperfectly known species of *Nautilus* from the Inferior Oolite, preserved in the British Museum (Natural History)," *Proc. Malacol. Soc.* iii. pp. 117-139, Dec. 1898. The observations of Willey on sexual dimorphism in the recent Nautilus have satisfactorily dispelled any doubts as to the existence of such a character, and divergences between individuals of any fossil species may therefore be interpreted as due to sex. Of the eleven species here described, seven appear to present both a broader form (male) and a narrower form (female) occurring at the same locality and horizon. In some specimens also it has been possible to trace very clearly the position of the anterior boundary of the muscular attachment. A specimen of *N. bradfordensis* shows the black layer as a band enveloping the whorl immediately in front of the aperture. A few non-adult specimens are described; and it is interesting to note that the British Museum specialist definitely accepts the approximation of the last two septa as a criterion of maturity.

A FALSE FOSSIL. J. S. DILLER. "Origin of Palaetrochis," *Amer. Journ. Science*, vii. 1899, pp. 337-342. In 1856 Professor Ebenezer Emmons described two species of Paleotrochis fr m the so-called Taconic rocks of Montgomery County, in North Carolina, and regarded them as siliceous corals, and as the oldest representatives of animal life upon the globe. But Hall, Marsh, J. A. Holmes, and others denied their organic nature, whilst C. H. White almost as strongly advocated it. Mr. Diller determines the Palaetrochis rock as an acid volcanic full of spherulites, and concludes "that Paleotrochis, where most perfectly developed and composed of granular quartz, is the result of deposition after the spherulitic growths about it and within it had developed, but whether this deposition followed soon after that of the spherulites in the course of solidification, or took place in hollow spherulites (lithophysae), or resulted perhaps long subsequently at the time of rock alterations, is not so clear." But this seems clear that the Paleotrochis is no reputable coral.

DIPLOSPONDYLY. W. G. RIDEWOOD. "Some observations on the caudal diplospondyly of sharks," *Journ. Linn. Soc.* (Zool.) xxvii. 1899, pp. 46-59. It is a well-known fact that in Selachian fishes the vertebrae of the tail are twice as numerous as the caudal segments as marked by the spinal nerves and the intermuscular septa. Dr. Ridewood reviews the facts and comes to the conclusion, "that the condition of diplospondyly in the tail of sharks is secondary, but of ancient date; and, further, that it is purely adaptive, being calculated to maintain a due proportion between length of centrum and width of body, without diminishing the length of the muscle-segments. In the region of the body from the cloaca to the caudal fin, the demand for increased flexibility is prepotent over the normal tendency of the chondrified chordal sheath to segment in such a way that the centra are as numerous as the myotomes."

TERATOLOGIA. BERTRAM C. A. WINDLE. "Ninth report on recent teratological literature," *Journ. Anat. Physiol.* xxxi. pp. 507-526. In this valuable record, for the continuation of which all biologists should be grateful, Prof. Windle gives a clear and terse summary of recent progress. He gives references to 83 papers, and arranges the results under the headings:—experimental, general, duplicity, head and neck, thorax, abdomen, genitalia, and extremities.

SOME NEW BOOKS.

THE SILURIAN ROCKS OF BRITAIN.

Memoirs of the Geological Survey of the United Kingdom: The Silurian Rocks of Britain. Vol. I. Scotland. By B. N. PEACH, F.R.S., A.R.S.M., F.G.S., and JOHN HORNE, F.R.S.E., F.G.S., with Petrological Chapters and Notes by J. J. H. TEALL, M.A., F.R.S., F.G.S. Royal 8vo, pp. xviii. + 749; xxvii. plates, 121 figures in the Text, and a coloured Map on the scale of ten miles to the inch. Published by order of the Lords Commissioners of H.M. Treasury, 1899. Price 15s.

FOR some reason that has not yet been discovered, the older rocks of Scotland appear to have been formed under somewhat different conditions from those which prevailed when rocks of the same age were in process of formation in other parts of the kingdom. Not only is this the case with regard to their original characters, but it is equally so with regard to their subsequent history. Nature's forces appear to have attacked the older rocks of Scotland more energetically than has been the case elsewhere; and, as a consequence, their present arrangement is much more difficult to make out than that of those, for example, which are in the Lake District. The Cambrian and Pre-Cambrian Rocks of Scotland have been deformed, and their order deranged, to an extent which is almost without a parallel outside of the Alps; and even those rocks which were formed between the close of the Cambrian period and the commencement of Devonian times have fared, in this respect, hardly any better than their predecessors. Hence the task of deciphering the geological history of the Ordovician and Silurian Rocks of Scotland has presented so many difficulties that it has repeatedly baffled the efforts of even the ablest geologists. It is quite true that each observer who has tried to work out the geological structure of these rocks has added something of value to the common stock of information; but it is now obvious to those who look back upon the methods of work adopted by these earlier geologists, that most of them had gone upon the wrong lines. As a consequence of this fundamental error, our knowledge of the succession of geographical events to which these rocks were due, proved to be almost as defective as was our knowledge of the sequence of biological events of which these rocks contain a record.

The reason why so many able men failed to read the history of these Scottish Ordovician and Silurian strata aright is sufficiently plain to us, now that our eyes are opened. It lay in the fact that, for some inexplicable reason, it has long been the fashion in Scotland to ignore the fact that geology is quite as much concerned with the past history of Life upon the Earth as it is with the physical history of the old sediments in which the vestiges of that life have been entombed. In the great majority of cases a student has been trained to regard the mineral constitution of some rock, let us say, for example, a dyke,

as a matter of vastly greater importance than the history of the fossils occurring in the strata which that dyke happens to cross. Whether the dyke consisted of basalt or of "melaphyre," or whether it should be called a dolerite or a "diabase," has in Scotland only too often been considered a question of far greater importance than whether the graptolites which occur in the strata traversed by that dyke indicate that the rocks are of Arenig age, or whether they date from Wenlock times, or, again, whether they represent any period of intermediate age. We cannot all be specialists, it is true; but, clearly, every modern geologist should be familiar with at least the zonal fossils of the rocks amongst which he is at work. One would also think that his work would prove of much greater interest to him if he knew something of the biological relationships of the organic remains with which he is likely to meet. As things stand at present, it may be confidently stated that, taking the whole of Scotland, the number of those who are really working at fossils of any kind may be counted on one hand—one of the authors of the present work being one of them. And even the number of those who are systematically making collections of fossils probably does not exceed a score. The case, of course, is very different south of the Border, where nearly every geologist takes a more or less keen interest in Palaeontology.

That these defects will soon be made good no one who carefully studies the most admirable historical introduction given in one of the earlier chapters of the book under notice can for a moment doubt. The whole of that history leads up to a triumphant vindication of the claims of Palaeontology to occupy a foremost place in the studies of all geological students in the future, not only on account of the light which that science throws upon the evolution of existing forms of life, but also on account of the invaluable aid it affords in unravelling the complicated structure of districts like that of Girvan, or of the Valentians or Southern Uplands of Scotland. Had it not been that Professor Lapworth brought to bear upon the rocks of these districts a combination of skill in field work with an extensive knowledge of Palaeontology, we should probably still have been no wiser regarding the true history of the rocks in question than we were thirty years ago.

On taking up the work whose contents have suggested these remarks, the reader will do well to give a full consideration to the section of the book referred to. He will find in it evidence of a strongly-marked desire on the part of the authors to deal in a generous spirit with the work of all previous observers, and he will further see how each man has added something of his own to our knowledge of these difficult rocks, and how that intellectual giant amongst geologists, Professor Lapworth, largely by working out the zonal distribution of the Graptolites, has enabled us, in the end, to gain a clear view of the true succession of the Scottish Ordovician and Silurian Rocks. By the light thus presented, Messrs. Peach and Horne, with Mr. Macconochie, have laboriously worked over the whole area where these rocks occur, and, bringing to bear upon them the results of wide experience, they have completed the survey of the whole area of which this book treats. It is from the vast mass of material collected in the course of this work that Mr. Horne has completed the present Memoir. No one who takes the trouble to read any section of it can fail to see that, in all respects, it forms a perfect model of what such a book should be. It may truly be said to present all that can be known at present regarding the geology of the group of rocks to which it specially refers, and Sir Archibald Geikie is to be congratulated on the production by his staff of a Survey Memoir in which the work of eminent specialists like Mr. Teall, Professor Lapworth, Dr. Traquair, as well as Professor Laurie and Mrs. Robert Gray, has been skilfully incorporated with the vast mass of information collected by the above-named members of the field staff of the Survey.

It may be well to mention here that the various geological maps, rock specimens, and most of the fossils, referred to in the Memoir, are exhibited in

the Gallery devoted to Scottish Mineralogy and Geology in the Edinburgh Museum of Science and Art.

It is no easy matter to give a summary of the contents of a book which contains in a highly-condensed (though perfectly lucid) form, so enormous an accumulation of facts. To the readers of *Natural Science* probably the chief interest of the work will centre upon the palaeontological portion, and upon such parts of the work as are more or less directly concerned with the Life of the Past ; but we may, nevertheless, briefly notice its contents as a whole :—

The earlier chapters of the history bring before us records of a submarine volcanic episode, during the latter part of which the chief organic remains which were entombed in the sediments belonged to the Tetragraptidae, Phyllograptidae, and a few other Arenig forms of graptolites, together with one or two Phylloped Crustacea, and a few Inarticulata, representing the Brachiopoda. Next follows a record of much deeper water conditions, during which a large area of what is now Southern Scotland would appear to have lain at the bottom of an ocean more than 2500 fathoms in depth. It was at this time that the now well-known Arenig Radiolarian Chert was formed. (It may not be generally known that Mr. Peach was really the first to recognise the true nature of this deposit, and that named specimens of it were exhibited in the Gallery of Scottish Geology and Mineralogy in Edinburgh a year or more before any published description appeared.) Following this ancient oceanic ooze comes a record of frequent oscillations of level, and of a gradual elevation of at least the western part of the district to above the level of the waves. In the meantime the Arenig forms of graptolites had died out, new generations of Rhabdophora had gradually come into being, and the conditions favourable for the evolution of group after group of new species and genera appear to have continued, in certain areas, as around Moffat, for an interval of time of incalculable length. Then follows another and lengthy period, during which we have perfectly clear evidence, in other areas, of the gradual appearance and disappearance of whole families of Coelentera, Brachiopoda, Trilobita, and Arthropoda, as well as of other organisms ; and evidently also (although the earlier chapters of this part of the history are yet wanting) of the gradual evolution of the ancestral forms of the Vertebrata. One of the most interesting features in the book is the record of the discovery of fish remains in the higher beds of the Silurian Rocks. These fossils have already enabled Dr. Traquair to throw a flood of light upon some points that had previously remained in obscurity ; and there can be little doubt that we shall shortly learn more still, as the beds that yielded these organisms continue to be diligently searched. The closing episode of the Silurian Period in Scotland was one in which the marine conditions which had so long endured gradually came to an end. Continental conditions took the place of oceanic, terrestrial volcanoes arose upon what had formerly been the sea-bottom, and the Silurian sea finally gave place to the deserts within which the Old Red Sandstone was formed.

It is chiefly in connection with the eruptive and metamorphic rocks which date from this Devonian period, that Mr. Teall's numerous and valuable contributions have been given. Like the Stratigraphical and the Palaeontological parts of the book this Petrographical part cannot be summarised, for the simple reason that, from beginning to end, the work is already as closely condensed as it can possibly be.

Regarding the book as a whole one may confidently state that it is the finest geological monograph that has yet appeared, at home or abroad, and that it reflects the highest credit upon every one concerned in its production.

J. G. G.

THE PROPER STUDY OF MANKIND.

Man Past and Present. By A. H. KEANE. Cambridge Geographical Series. Pp. xii. + 584, with 12 plates. Cambridge University Press: C. J. Clay & Sons. 1899. Price 12s.

Linguistic and literary attainments are as essential to the specialist in the field of Ethnology, as keen-edged tools are to the skilled artizan. A perusal of "Man Past and Present," by Prof. Keane, amply proves that, in addition to these accomplishments, the author is conversant with the vast amount of anthropological literature which has come into existence since the banner of Evolution was first raised by Darwin and Wallace some forty years ago. The volume now before us is the second which has appeared within the last few years from the pen of Mr. Keane on the same fascinating subject. The first, under the title of "Ethnology" (1895), was upon the whole well received by general anthropologists, although several critics pointed out its inadequacy to supply the recognised want of a compendious handbook to Ethnology in the English language. The subject-matter was treated in two divisions—(1) Fundamental ethnical problems, and (2) the primary ethnical groups—the first being unnecessarily long, and the second irritatingly short, and altogether unsatisfactory. The present volume furnishes, at least to some extent, the deficiencies of the former. But unfortunately in avoiding Scylla the author has fallen into Charybdis, by having to repeat in his new book much of what had already been said. In "Ethnology" the ethnical groups (less than half the volume) are discussed under *Homo Aethiopicus*, *H. Mongolicus*, *H. Americanus*, and *H. Caucasicus*. In "Man Past and Present" the subject is continued in several chapters on "Negroes," "Mongols," "American Aborigines," and "Caucasic Peoples." It is like an author who, having four tales to relate, and finding that he could not do so in one volume, publishes the first half of each tale in one book, and the concluding portions in a second book, both volumes being actually under different names. We greatly regret this disposition of the materials, as we are convinced that by a little re-arrangement of the anthropological problems, together with a curtailment of lengthy disquisitions on secondary details, so as to bring them more into harmony with the ethnological section, Mr. Keane had the opportunity of producing one book which would, undoubtedly, have been a great boon to students.[1] Moreover, both volumes are weakened by a division of the illustrations. We have, however, pleasure in quoting the following remarks from the preface which, while explanatory of the *raison d'être* of two separate books, gives an excellent résumé of the contents of the volume, as well as a specimen of the author's style:—

"In the preface to the 'Ethnology' a promise was held out that it might be followed by another dealing more systematically with the primary divisions of mankind. The present volume appears in part fulfilment of that promise. In the 'Ethnology' were discussed those more fundamental questions which concern the human family as a whole—its origin and evolution, its specific unity, antiquity, and primitive cultural stages, together with the probable cradle and area of dispersion of the four varietal divisions over the globe. Here these divisions are treated more in detail, with the primary view of establishing their independent specialisation in their several geographical zones, and at the same time elucidating the difficult questions associated with the origins and inter-relations of the chief sub-groups, and thus bridging over the breaks of continuity between 'Man Past and Present.'

"The work is consequently to a large extent occupied with that hazy period vaguely called pre-historic, when most of the now living peoples had already

[1] Such an ideal work already exists in the French language in "Les Races Humaines," by Dr. R. Verneau.

been fully constituted in their primeval homes, and had begun those later developments and migratory movements which followed at long intervals after the first peopling of the earth by pleistocene man. By such movements were brought about great changes, displacements, and dislocations, involving fresh ethnical groupings, with profound modifications, or even total effacements of racial or linguistic characters, and complete severance from the original seats of the parent stocks. In some cases the connecting ties are past recovery, so that the ethnical, like the geological, record must always remain to some extent a mutilated chapter in the history of the world and of humanity. But in our times many of the more serious gaps have been often most unexpectedly made good by the combined efforts of philologists, physical anthropologists, and especially archaeologists, who have come to the welcome aid of the palethnologist, hitherto groping almost helplessly in this dark field of human origins."

Mr. Keane is a "monogenist," and maintains that all the varieties of the human race can be traced back to one centre of evolution. The first splitting of the main stem was almost simultaneously into the three types—Negro, Mongol, and Caucasian—which still represent mankind on the globe. *Homo Americanus* is a great puzzle to ethnologists, more especially as the tendency of the most recent investigations is decidedly against the theory that palaeolithic man of quaternary times ever existed on the North American continent. By successive divergences from these three primary branches under the moulding influences of cross-breeding, and climatal, geographical, and other changes in the environment, Mr. Keane accounts for all the varieties of shadings which characterise and distinguish the present inhabitants of the globe. The "cradle-land," from which *Homo sapiens* first emerged and bade farewell to his congeners of the brute creation, was, according to the author, a lost continent, "Indo-African," now represented only by Madagascar and a few islands in the Indian Ocean. Of the three divisions of mankind still living, the Negroid ("Negrito") type is regarded as most nearly approaching the original form of tertiary man. On the *modus operandi* of this primary stage of humanity he quotes from Dr. Munro's writings on the influence which the erect posture played in the higher development of the brain, with regard to which he states (page 7):—"This greatly strengthens the view always advocated by me that man began to spread over the globe after he had acquired the erect posture, but while in other physical and in mental respects he still differed not greatly from his nearest akin."

The three chapters dealing with the Caucasic peoples will be found of greatest interest to general readers of anthropology. Here some of the more burning problems of the hour, bearing on early European civilisation, are intelligently discussed; nor does the author by any means submerge his own individuality in the various controversies which he summarises for his readers. He follows Prof. Sergi in assigning the Iberians, Ligurians, Pelasgians, etc., to an original home in North Africa. The "Mediterranean race," from whom a stream of "migration set steadily and uninterruptedly into Europe throughout both Stone Ages," was dolichocephalic, short in stature, and of a dark brown colour.

The task which Mr. Keane has set before himself in the compilation of this most readable book is one which few anthropologists would undertake, and which still fewer are competent to execute. He gathers his materials, apparently with great linguistic facilities, from far and wide—not always, however, from the original investigators, who are too often allowed to disappear, while the second-hand compilers are brought to the front. But, in extenuation, this much must be acknowledged, that his authorities are most faithfully given—and this is one of the most valuable features of the book. Scarcely a subject in the whole range of Anthropology and pre-historic Archaeology is omitted by this versatile author. Archaeologists, geologists, philologists, folklorists, and even modern globe-trotters come on and go off the stage with startling suddenness.

Yet, amidst the diversified and world-wide dramas thus depicted in a long series of bygone civilisations, the author moves with much freedom and elasticity, bestowing here and there, as the case may be, a word of praise or dispraise. Altogether, Mr. Keane's book (of course including its predecessor as an integral part) is to be highly commended, not only on account of the general soundness of the opinions upheld, but also because of the interesting manner in which he has marshalled his facts. Nor will beginners in the study of Anthropology object to read the two volumes, notwithstanding a certain amount of repetition, for in both the author carries with him the attention of intelligent readers.

R. M.

THE ZOOLOGISTS IN CONGRESS.

Proceedings of the Fourth International Congress of Zoology. 8vo. Pp. xv. + 422, 15 pls. London, 1899.

This bulky volume forms no exception to the rule that the official "Proceedings" of Societies or Congresses are usually somewhat disappointing. It is true that the value of an international meeting of zoologists can hardly be estimated by that of brief abstracts of papers and speeches, but it is difficult to avoid a slight feeling of disappointment that the personal contact of so many specialists should produce apparently so little result, and that so many of the discussions should end in nothing.

On general subjects one of the most interesting papers is that by Prof. Mitskuri on zoology in Japan. In a brief historical sketch of the progress of natural science in that country, he shows that the common belief in its sudden rise within recent years is quite unfounded, and that the present condition of affairs is merely the natural outcome of generations of preparation. From the interesting account of scientific education at the present day in Japan we cull one little fact only. The biological students of Tokyo University are required to spend *at least* one season at the Marine Station in connection with the University, while those who take up zoology as a speciality spend much more time than this at the seaside. We recommend this regulation to the notice of some Western Universities.

Of the general discussions those on the position of sponges and on the origin of mammals are reported in some detail. As to the sponges there seems practical unanimity that they are not Coelenterates, but there is more doubt as to whether they are to be regarded as a separate phylum of the Metazoa, or as having originated from the choanoflagellate Protozoa independently of the other Metazoa. The position adopted depends upon the views held as to the meaning of the reversal of the germinal layers during metamorphosis, but the discussion of this point when pushed to extremes largely resolves itself into a juggling with words.

The discussion of the origin of mammals contains much that is interesting. While Professor Haeckel still adheres to the earlier position that the placentals are descended from a marsupial stock, most other zoologists seem to regard Hill's discovery of a deciduous allantoic placenta in *Perameles* as conclusive proof that placentals and marsupials have arisen from a common stock and form parallel phyla. As to the more remote ancestry there is much more doubt and great difference of opinion. Prof. Osborn believes that mammals arose from the theriodont division of the anomodont reptiles, and that they are diphyletic, the marsupio-placental stock arising at the time when the Theriodontia conserved a number of Amphibian characters. Prof. Seeley, on the other hand, believes that anomodonts are not the ancestors of mammals, but that both originated from a common unknown stock. On the other hand, Prof. Marsh rejected the suggestion of reptilian affinities altogether, and looked for the ancestors of mammals among early amphibians. All were agreed

in placing the point of origin far back, in Silurian or Devonian times, so that there is a certain fitness in the closing speech, that of Mr. Sedgwick, in which "pre-Cambrian times" are suggested as the period of origin, not of mammals only, but of all the "great classes of the animal kingdom." Mr. Sedgwick suggests that "the main part of the evolution of organisms must have taken place under totally different conditions to those now existing, and must remain for ever unknown to us." We duly altered our belief in Recapitulation to meet Mr. Sedgwick's criticisms, and have learnt to hold the cell-doctrine lightly at his bidding, but this new instance of "thätige Skepsis" makes so heavy a demand upon our credulity that we prefer to regard it as a delicate piece of sarcasm.

Among other interesting papers is one by Messrs. Mesnil and Caullery on polymorphism, and the occurrence of epitokous forms in the common littoral annelid *Dodecacaria concharum*. They find that the common form (Form A) is viviparous, and apparently reproduces parthenogenetically; males at least have not been found, and reproduction takes place at a time when the males of the other forms are not yet ripe. The second form (Form B) is rare, and occurs in both atokous and epitokous forms. The modifications of form displayed are in all respects similar to those displayed by the Nereids and Syllids. The epitokous forms leave their tubes and become free-swimming. Very rarely a third form was found (Form C), which likewise becomes epitokous, but the changes are less marked than in B. Of this form females only were found. The authors are uncertain whether these forms are to be regarded as allied species or as constituting a polymorphic species. The point of special interest is that the phenomenon of epitoky has not previously been described in sedentary Polychaetes. It seems probable that it occurs much more frequently among Polychaetes than is at present suspected.

The volume is furnished with a bulky appendix, a considerable portion of which is taken up by "Correspondence on the Nomenclature of Lepidoptera," being the classified answers to questions circulated among certain entomologists by Sir George Hampson. Whether this will advance the science of entomology or not, we cannot undertake to say, but it can be confidently recommended alike to the psychologist and the student of human nature. If, as we are led to believe, systematic or other work is almost impossible to the entomologists, on account of the difficulties of nomenclature, there seems no reason why they should not occupy their time instead in classifying the views of their fellow-workers on various subjects, but the result seems slightly ludicrous to the onlooker.

The appendix also contains in full Prof. Hubrecht's paper on the "Development of the Placenta in *Tarsius* and *Tupaia*, with Observations on its Importance as a Haemopoietic Organ," which is fully illustrated by plates. The volume contains abstracts of numerous other papers in addition to those mentioned, but most of these have been previously published elsewhere.

N.

INSECTS.

Insects (Part II). By DAVID SHARP, M.A., M.B., F.R.S. Being Vol. VI. of the Cambridge Natural History. Edited by S. F. HARMER and A. E. SHIPLEY. Pp. xii. + 626 with 293 figures. London: Macmillan, 1899. Price, 17s. net.

A hearty welcome will be given by all students of insects to this concluding portion of Dr. Sharp's monumental work, the commencement of which appeared four years ago in the fifth volume of the "Cambridge Natural History." The volume now before us deals with the higher Hymenoptera, the Coleoptera, the Lepidoptera, the Diptera, the Thysanoptera, and the Hemiptera. It must be admitted that this arrangement of the orders of insects is unsatisfactory; the Lepidoptera, for example, are removed far from their allies the Trichoptera

(included among the Neuroptera in Part I.), and placed next to the Beetles, with which they have no near relationship.

The treatment of the various groups is, however, admirable. No fewer than 180 pages are devoted to the Bees, Wasps, and Ants, and the external form and habits of these most interesting of insects are fully described after the observations of Janet, Verhoeff, Marchal, Wasmann, and other recent naturalists. Internal structure should perhaps have received more attention; some details of the digestive and reproductive systems of the honey-bee might fairly have been expected. Dr. Sharp writes on the economy of the social insects with charming enthusiasm, freshness, and human interest. After recording Hoffer's confirmation of Godart's statement—made 200 years ago—"that a 'trumpeter-bee' is kept in some nests to rouse the denizens to work in the morning," the suggestion is hazarded that the hour when the trumpeting occurs (3 or 4 A.M.), caused the observation to remain discredited for two centuries! The section on ants and their ways is particularly good.

Most of Dr. Sharp's own entomological work has been done on the Coleoptera, and his account of this order will therefore be scanned with special interest. Undoubtedly some grouping of the numerous families of beetles into large divisions is very convenient and desirable. Our author adopts the well-known Lamellicornia (placed at the head of the order), Adephaga, Heteromera, Phytophaga, and Rhynchophora, while the many families which will not fit into any of these—the Clavicornia and Serricornia of former writers—are relegated to a group appropriately called the Polymorpha. The account of each family is illustrated by a figure of a typical species with its larva; an original figure of the remarkable stridulating-organ of a Passalid grub (p. 192) is worthy of special mention. The enigmatic Strepsiptera are doubtfully regarded as an aberrant group of Coleoptera.

The section on the Lepidoptera is full, more attention than usual being devoted to internal structure. In the account of the wing-nervuration it is a pity that the American nomenclature—familiar to readers of *Natural Science* through the papers of Mr. A. R. Grote—is not mentioned. In classification, Sir G. Hampson is followed, his key to the families from the "Moths of India" being reproduced in full. Dr. Sharp's views on protective coloration and mimicry are far from "orthodox." It is doubtless well that the Batesian and Müllerian theories should not be dogmatically preached as they have been by many writers. At the same time, Dr. Sharp is hardly as fair as usual when he writes, "We think it is clear that the explanation from our point of view is of but little importance," and when he refers to Prof. Poulton's "Colours of Animals" as "the case as stated by an advocate." Dr. Dixey's recent suggestive work in support of the positions attacked is not mentioned.

That most difficult order of insects, the Diptera, is next dealt with, and the account of the outer form, classification, and larvae of flies is admirably clear and well balanced, though the internal organs and the formation of the parts of the imago in the grub and pupa might well have received more attention. The Fleas are treated as a sub-order of Diptera. There is a good account of the small but interesting group Thysanoptera, which is rightly regarded by Dr. Sharp as forming a distinct order. In the reference to Uzel's recent beautiful monograph on these insects, it is implied that the work is entirely in Bohemian, whereas it contains a rather full German summary.

The concluding chapter, devoted to the Hemiptera, is admirable both in its morphological and systematic portions. The Lice (Anoplura) are doubtfully treated as a sub-order. The volume is beautifully illustrated, and the footnote references to literature are full and instructive. Indeed, little complaint can be made except to "ask for more." Could not the author have added a chapter giving us his views on insects as a whole, the relationships between their orders, the probable course of their evolution? Only the faintest echoes are to be found in this book of the bold and suggestive paper on insect classification read

by Dr. Sharp last year at Cambridge before the International Zoological Congress. Here he restricts himself to a record of the facts of insect life and structure, and perhaps by the absence of any trace of a phylogenetic tree he silently rebukes the rashness of younger men. GEO. H. CARPENTER.

DR. WILLEY'S RESULTS.

Zoological Results based on Material from New Britain, New Guinea, Loyalty Islands, and elsewhere. Collected during the years 1895, 1896, and 1897. By ARTHUR WILLEY, D.Sc. (Lond.), Hon. M.A. (Cantab.) Part III. pp. 207-356, pls. xxiv.-xxxiii. Cambridge University Press, 1899. Price 12s. 6d.

Part III. of Dr. Willey's "Zoological Results" contains articles by Dr. Gadow, Mr. Shipley, and the author. Dr. Gadow gives an interesting account of the variations to be found in the carapace of young chelonians. We must assume that the course of evolution in the chelonian branch of reptiles has been in the direction of a steady reduction in the number of scutes covering the carapace, in accordance with a "widespread evolutionary law" of the "specialised few" replacing the "generalised many."

The turtlets show a greater percentage of abnormalities in the carapace than the older individuals. "Our Turtlets start with many, with at least 24 dorsal scutes (leaving out the marginals), and then reduce them to 16. In other genera the reduction has advanced to 14, to 13, and individually to 12. This means onward development. The ideal, the goal for the young *Caretta*, is the possession of a 16-scuted shell. Those which start with 24 perhaps never reach the ideal, but this failure does not seem to hurt them, natural selection remains indifferent. Others start with 22, 21, 20, 19, or 18 scutes, and the latter individuals are rather common in the newly-hatched stage, and all of these seem to reach the goal. . . . These variations from the normal type all lie in the direct line of descent, and the more serious the variation the farther back it points. Moreover, the changes necessary to turn any given variation into another one less abnormal, until ultimately the normal condition is reached, are not erratic, but stand in strict correlation with each other, and proceed strictly on definite lines. I therefore call this kind of atavistic variation *orthogenetic*." This orthogenetic variation in young chelonians appears to be a very striking example of Van Baer's law in its modern application.

Dr. Willey follows with a valuable contribution to our knowledge of the Enteropneusta. Firstly, he gives a synopsis of the groups under the three families of Ptychoderidae, Spengelidae, and Balanoglossidae, followed by a detailed description of *Ptychodera flava*, *P. carnosa* n. sp., *P. ruficollis* n. sp., with *Spengelia porosa*, Willey, and *S. alba* n.sp., with notes upon the West Indian species *Pt. biminiensis* n.sp., and *Pt. jamaicensis* n. sp.

There are many interesting points upon which one could dwell in these descriptions, but space will not permit. *Spengelia* appears to offer some remarkable features, including the so-called vermiform process of the stomochord (the latter is a useful name suggested by the author for the "notochord" of the Enteropneusta), and the presence of truncal canals. Dr. Willey finishes his paper by a discussion of the "Morphology of the Enteropneusta." He propounds a theory of the origin of gill-slits, based principally on their relationship to the gonads in this group.

Gill-slits primarily arose as inter-zonal depressions between the zonary, metamerically repeated gonads, functioning for the oxygenation of the gonads. Later they acquired openings into the pharyngeal wall, and were used for the respiration of the individual.

Further, he comes to important conclusions with regard to the stomochord of Enteropneusta and related organs in *Cephalodiscus* and Actinotrocha, which

cannot be dealt with here, especially as they are more fully stated elsewhere. He restates his former well-known conclusion of the homology of the vertebrate thymus with the branchial tongue-bars of Enteropneusta, and further finds the homologue of the endostyle in the parabranchial ridges, paired ciliated tracts which pass forwards to unite with the epibranchial band. This suggestion may be further compared with Garstang's comparison of the echinoderm ad-oral band with the endostyle.

Enough has here been said to show the value of Dr. Willey's contribution.

The third memoir is by Mr. Shipley, who takes the occasion to give a systematic revision of the groups of Echiurids. *Bonellia viridis* and four species of *Thalassema* are comprised in Dr. Willey's collection. The author gives a useful summary of the most valuable specific characters, of which the number of nephridia and the enumeration of muscle bundles appear the most important. The five genera, *Bonellia*, *Echiurus*, *Hamingia*, *Saccosoma*, and *Thalassema*, are dealt with.

From these brief remarks it will be noted that Part III. of the "Zoological Results" is full of interest alike to the morphologist and the systematist, and the author is to be congratulated upon his own labours and upon the able assistance which he has obtained. A. T. M.

REASONING MADE SIMPLE.

The Psychology of Reasoning, based on Experimental Researches in Hypnotism. By Dr. ALFRED BINET. Translated by A. G. WHYTE, B.Sc. 8vo, pp. 191. Chicago: The Open Court Publishing Company, 1899. Price 3s. 6d.

Dr. Alfred Binet's name is well known in association with that of Dr. Charles Féré (placed on the dedication page of this little book), to all who are interested in the phenomena of hypnotism. He here makes these phenomena throw such light as they can on the psychology of reasoning. His treatment has the advantage of perfect lucidity and of a simplicity which is, we venture to think, delusively alluring.

Reasoning is not regarded by Dr. Binet as a specialisation of conscious activity, and a differentiation only reached at a late stage of mental evolution, but rather as the general form of all psychical life. "To sum up," we are told, "all forms of mental activity are reducible to a single one—reasoning." "Three images which succeed each other, the first evoking the second by resemblance, and the second suggesting the third by contiguity—that is reasoning. Submit any reasoning to analysis, and you will find nothing else at the bottom of the crucible. But it would be an error to believe that this process belongs specially to reasoning. Far from it. We meet with it in all intellectual operations; it is the single theme upon which nature has embroidered the infinite variations of our thought." When a three-day-old chick avoids a cinnabar caterpillar as the result of previous experience of like objects, we have the three successive images; this caterpillar evoking images of certain others by resemblance, and these others suggesting the nastiness which was unpleasantly contiguous. Changing for convenience the order of formulation, and leaving out one little word, Dr. Binet gives for comparison—

> This is a crystal;
> All crystals have planes of cleavage;
> This has a plane of cleavage.

Here, he says in effect, this crystal is on all fours with this caterpillar; other crystals suggested by resemblance take the place of other caterpillars similarly suggested; while experience suggests cleavage in the one case just as it suggested nastiness in the other. But where does the *therefore* come in? In the

present state of psychological nomenclature it seems open to an author to define any term in accordance with his special predilections. We think, however, that the majority of reasoning men believe that the process demands a due comprehension of that subtle relationship among thoughts which we symbolise by ∴ or ∵ But this is perhaps because it is consonant with our own special predilections. C. Ll. M.

A WELCOME WORK.

The Origin of the British Flora. By CLEMENT REID, F.R.S. 8vo, pp. vi. + 191. London: Dulau and Co., 1899.

Few works on the British flora possess greater interest or importance than this, which deals with the evidence gained during recent years from investigations into the vegetable remains of the later Tertiary and the Post-Tertiary deposits in Britain. These investigations rest mainly on the work of Mr. Reid himself, ably supplemented by Mr. James Bennie and other careful observers. Their results have been published through varied channels; and Mr. Reid has laid all interested in the flora of Britain under an obligation by bringing these results, and a good deal of other information, within easy reach. The author is peculiarly well fitted to perform such a work. Long-continued personal researches in Britain have been supplemented by wide acquaintance with the labours of others, both in Britain and throughout the north of Europe. He has produced a book that will do much to stimulate others to extend the work and to fill the gaps in the record in so far as that can be done. One part Mr. Reid might have extended with advantage to the recruits that the book is likely to enlist. The hints that he has given as to the most productive localities, and the methods of preparation of plant remains from the Tertiary and the Post-Tertiary deposits in Britain, make one feel how helpful a fuller treatment of both topics would have been. His remarks about the difficulty of obtaining fruits and seeds of existing plants with which to compare the fossils, emphasise strongly how imperfect herbaria are, as a rule, in the provision of complete examples of these parts.

The introductory chapters deal with the leading peculiarities and divisions of the existing British flora, the means of dispersal of the seeds met with among its members, and their consequent fitness for ready distribution; the changes in the form of the islands and their relation to the continent of Europe in former periods, and the evidences of changes of climate and their influence on the flora. A careful study of these chapters will aid much in arriving at clear views of the true nature of the problems involved in explaining "the origin of the British flora," and in accounting for its more marked peculiarities when compared with the floras of the adjoining countries.

Next follows an enumeration of the various localities in Britain (arranged alphabetically) from which these fossils have been recorded, with a notice of the probable age of each deposit, and a list of the species identified from it. Some continental localities are similarly treated. Then comes a list, in systematic order, of all existing British plants that have been identified as fossils, with a list under each of the localities in Britain in which it has been found fossil, or on the European Continent, if not yet found fossil in Britain; and the age of each plant as a fossil is given. The chief facts under this are briefly summed up in a "Table showing the range in time of the British Flora."

It is no mere form of words to say that the book is indispensable to all who wish to gain a clear conception of the nature of the British flora. This is evident from a single perusal of its pages; but its full value will be realised only after frequent and continued reference. Only six species, no longer found in a wild state in Britain, have as yet been identified with certainty as living in our islands in the later Tertiary or Post-Tertiary times. These are:—*Acer monspessulanum*, *Trapa natans*, *Salix polaris*, *Picea excelsa*, *Naias graminea*, *N. minor*. A

number of others are indicated by seeds or other remains that have not yet been determined, and there is evidently much work to be done in the field of study so well opened by Mr. Reid. J. W. H. TRAIL.

MICROSCOPY FOR BEGINNERS.

Chats about the Microscope. By HENRY C. SHELLEY. 8vo, pp. 101 (8 blank). The Scientific Press Ltd., London, 1899. Price 2s.

This is a nicely written and nicely printed little book, beginning with a brief account of the compound microscope, methods of mounting, etc., and going on to descriptions of various objects living, and otherwise suitable for examination. The descriptions are rather flowery than detailed; the lines are "heavily" leaded (*Anglice*, wide-spaced) to correspond with the extreme meagreness of the text. It belongs to a type nearly extinct; and, on the whole, we think it would be nearly as welcome a gift-book to a lad fond of natural history as Wood's "Common Objects of the Microscope," and more up to date. Most of the 30 figures are at least fair, but the plate of the hyaline *Stephanoceros* is nearly as grimy as one of Phil May's "Three Black Pearls," and the lovely *Micrasterias Crux-melitensis* is vilely caricatured. Still we think that it may have a fair sale through the opticians. M.

A PROFESSOR OF PHYSICS DEALS WITH ORGANIC EVOLUTION.

Die Enstehung des Lebens aus mechanischen Grundlagen entwickelt. By Dr. LUDWIG ZEHNDER, A. O. Professor of Physics in the University of Freiburg i. B. Erster Teil. Moneren. Zellen. Protisten. 8vo, pp. 256, with 123 figs. Freiburg i. B.: J. C. B. Mohr (Paul Siebeck), 1899. Price 6 marks.

The author has previously endeavoured in his "Mechanik des Weltalls" to refer all known physical and chemical forces to gravitation; and he here attacks the problem of life. From atoms he leads the reader gently to molecules, and from molecules to "Fistellen" (molecules aggregated in hollow cylinders), and before we quite know where we are we have reached the Protists. On the ascending path, the gradient of which has been skilfully made easy, our confidence is increased by two fundamental biological principles: the first, that substance endeavours to multiply; the second, that substance endeavours to adapt itself to the conditions of existence. It need hardly be said that the molecules and fistellae multiply in nutritive conditions, and have their struggle for existence like full-fledged organisms. A full discussion of the soul is reserved for the third part of the book. Perhaps by that time the learned author may have realised that the organism is not so simple as his theory suggests. In particular, we should desire more detail in regard to the origin of its power of adapting itself. X.

CHILD-STUDY.

Anthropological Investigations on One Thousand White and Coloured Children of both Sexes, the Inmates of the New York Juvenile Asylum. By Dr. ALES HRDLICKA. 8vo, 86 pp. New York, 1899.

The principal aim of these investigations is to learn as much as possible about the physical state of children who are being admitted to and kept in juvenile asylums. In the second place, this study is a part of the general anthropological work of the author, which is expected to result in an addition to our knowledge of the normal child, and of several classes of children who are, morally or other-

wise, abnormal. Cases where the parents were known have also furnished some data in regard to inheritance. The work has been carefully done, and the author's scientific temper is indicated by his refraining at present from any generalisations. We would echo his recommendation that the State Boards, and here as well as in America, should give their official sanction and support to such studies (without which our ameliorative devices will linger long on an empirical level), and should extend them gradually to correctional and other institutions, provided, of course, that the services of expert and unprejudiced investigators can be secured.

A PRACTICAL COURSE ON CYTOLOGY.

Praxis und Theorie der Zellen- und Befruchtungslehre. By Dr. VALENTIN HÄCKER, A. O. Professor in Freiburg i. Br. 8vo, pp. viii. + 260, with 137 figures. Jena: Gustav Fischer, 1899. Price 7 marks.

This book had its origin in the practical course of studies on the cell and fertilisation given in the Zoological Institute at Freiburg i. Br. Experience was thus gained in choosing the best material to illustrate particular points, and Dr. Häcker has made this available to other workers. The result is a practical handbook of great utility. It consists of lessons for sixteen days, and deals with forty objects, such as staminal hairs of *Tradescantia*, epidermis of salamander larva, *Amoeba* and *Pelomyxa*, *Stylonichia mytilus*, living nuclei from the bladder wall of the salamander, ovarian ova of newts, spermatozoa of the salmon and trout, leaf-epidermis of *Leucojum*, *Stentor coeruleus*, root-hairs, ovarian tubes of insects, corneal epidermis, testes of salamander, ova of *Ascaris*, *Thysanozoon*, *Canthocamptus*, *Anodonta*, *Myzostoma*, *Tegenaria*, *Echinus*, etc., hybrid larvae of sea-urchins, antherozoids of ferns, and so on. In each case the methods to be followed are clearly indicated. The lessons are intended to illustrate the structure of the cell, cell-division, oogenesis, spermatogenesis, reducing divisions and maturation, fertilisation, etc., and short discussions are interspersed dealing with the established facts and the current theories. Brief historical sketches of the progress of research are also given, and carefully selected references to literature. A brief general chapter on the cell concludes the volume. Opinions may differ as to the choice of objects, but all will probably agree that it was a happy thought on Dr. Häcker's part to place the results of his experience at the disposal of workers in other schools.

J. A. T.

THE FRIEND OF THE FISHERMAN?

The Lancashire Sea Fisheries: A Lecture delivered in the Chadwick Museum, Bolton. By C. L. JACKSON, M.Inst.C.E., etc., Presid. of Bolton Microsc. Soc. Pp. vii. + 85. Manchester: Abel Heywood and Son. London: Simpkin, Marshall and Co., 1899. Price 2s.

This was probably an amusing lecture to listen to, and interesting because of the personal reminiscences; but, unfortunately, the author has been induced by friends (so he tells us) to rush into print, and the little book, we fear, will serve no useful purpose and may be mischievous. Twenty to thirty years ago Mr. Jackson was evidently active as a fisherman and observer. He quotes from Buckland and Walpole, "Land and Water," and Reports of Fisheries Commissions of that date; and for him these statements are evidently conclusive, and the investigation of the sea which has been carried on since by nearly every civilised country either does not exist, or is only a fit subject for scoff and sneer.

The book is a venomous attack upon the Lancashire Sea Fisheries Committee, their methods and their administration, and is evidently written from

the point of view of one section of the fishing community—the shrimpers. The "Friend of the Fisherman" is much in evidence, and nothing is too bad for those who propose fishery regulation. Mr. Dawson and Dr. Herdman come in for a large share of the abuse.

The bane of so much "popular" fisheries literature at the present day (perhaps it was always so) is that the writers seem to think solely of what would be best for this or that set of men with whom they happen to have sympathy, instead of considering what is required in the interests of the public as a whole, not this year nor next, but for years to come. Y.

PETROLOGY FROM COOLGARDIE.

The Geology of the Coolgardie Goldfield. By Torrington Blatchford, B.A., F.G.S. Geological Survey of Western Australia, Bulletin No. 3. Perth, 1899. Pp. 98 and 2 plates.

This publication opens with a short account of the boundaries and history of the Coolgardie Goldfield, together with a statement of the opinions entertained by previous observers on the geology of the district. Then follow the author's personal observations. He cites Mr. T. A. Rickard's description of the deposits, in which the auriferous cement, having an average thickness of 2½ feet, is stated to rest upon a surface of decomposed granite. A capping of kaolin and sand-rock, the latter with seams of pipe-clay, rests upon the cement, this capping barely exceeding the thickness of the latter. The cement is less coherent than the "Banket" of South Africa. The observations of Mr. Göczel on these deposits are also quoted. The Kanowna Lead is described and its output given, the total yield of gold being estimated at 191,478 oz. 10 dwt. 22 gr. The ironstone gravel beds are next described, and then follow very admirable accounts of the granite, amphibolites, diorites, andesites, and schists of the district. The author is of opinion that the schists, which are hornblendic, or occasionally talcose, result from the surface weathering of amphibolites, and he adds: "As regards the amphibolites, there is little doubt in my opinion that they are so closely associated with the diorites as to be inseparable from them." The question of water-supply with details of borings is next dealt with, followed by important but concise descriptions of reefs. A couple of pages are devoted to an account of minerals found associated with the ore-bodies. Pages 51 to 78 give descriptions of the mines of the district. The remainder of the work is occupied by statistics, a diagrammatic representation showing the annual output of gold, and a coloured geological map of Coolgardie. Altogether this little publication is an admirable piece of work, one of which any survey might be justly proud, for besides being a treatise of great utility to a mining population, it is also a valuable contribution to petrology.

F. R.

THE LINNAEAN NAMES.

An Index to the Generic and Trivial Names of Animals described by Linnaeus in the 10th and 12th editions of his "Systema Naturae." By Charles Davies Sherborn. Manchester Museum, Publication 25. 8vo, pp. viii. + 108. London: Dulau and Co. Manchester: J. E. Cornish, 1899. Price 3s. 6d.

It should be known to zoologists that the author of this Index has for some years been engaged in the compilation of an "Index Animalium." Pecuniary aid has been received from the British Association and from the Zoological Society of London, and we understand that nearly all zoological writings from 1758 to 1800 inclusive have been worked through, and that the names contained therein have been entered in duplicate on a slip-catalogue. It is hoped

that the question of printing and publishing this portion of the accumulated material will soon be ripe for discussion. Meanwhile the book before us, published by the enterprise and liberality of the Manchester Museum, serves as a *ballon d'essai*. It is in itself a work of much utility, and it shows the method that will be followed in the larger "Index Animalium." From that, however, the present index differs in the omission, as unnecessary, of the author's name (*e.g.* Linnaeus, "Syst. Nat.") after each item, as well as of any indication to what class of the animal kingdom each genus belongs.

Such a work scarcely lends itself to criticism. The text appears to us both clear and accurate. Mr. Sherborn has indexed the sponges, which are omitted from the German Zoological Society's reprint of the tenth edition. He has included the numbers which indicate the position of each species in its genus, a matter of some importance. In an Introduction he gives an annotated list of the editions of the "Systema Naturae," and points out the changes involved by accepting the tenth instead of the twelfth edition as the *ab urbe condita* of systematic zoology. Among these appears the name of the Dodo, henceforward to be known, not as *Didus ineptus*, but as—well, buy the book and find out!

We have received a descriptive Catalogue of the Tunicata in the Australian Museum, Sydney. N.S.W., by Prof. W. A. Herdman (8vo, xviii. and 139 pp., with 45 plates; Liverpool, 1899). It is what it professes to be, a descriptive catalogue, and not a monograph, but its usefulness is increased by an introductory account of the structure and life-history of a typical Ascidian, and by a list as complete as possible of the Tunicate fauna of Australian seas. The Trustees of the Museum were fortunate in securing the services of Prof. Herdman, who is one of the highest authorities on Tunicata, and the catalogue will be welcomed by zoologists at home as well as in Australia. The liberal allowance of plates adds greatly to the value of the work.

In *Science* for June 30 there is an interesting short article by Mr. Sylvester D. Judd, on birds as weed destroyers. "The goldfinches and native sparrows are more beneficial to agriculture than a number of other species, such as the English sparrow and blackbirds, which at times injure grain and fruit, but there are some fifty species of birds engaged in the work of weed-seed destruction, and the number of species of weeds which they tend to eradicate amounts to more than three score."

In the scientific section of the current number of *The Literary Digest*, which is conspicuously up-to-date, there are translations of papers on the alleged germ of cancer (Bra's organism); on how to make coloured people white (E. Gautier) by "depigmentising" them electrically—a paper which shows that the Ethiopian may at considerable expense and with no obvious utility change his skin; on the age of the Niagara Falls (Prof. G. F. Wright); on experiments as to the sensitiveness of school children, by that arduous worker Dr. Arthur Macdonald; and more besides.

In the number of the *Scientific American*, dated July 8, Dr. E. Murray-Aaron tells of the habits of the "honey-birds" which guide explorers to stores of honey, but with their own gratification for their "end and aim." It is also noted in the same number that some of the insects which pollinate the Smyrna fig have been made to establish themselves in California. The flavour of the "fruit" is said to depend upon the number of ripened seeds.

In *Science* for July 7 there is an excellent lecture by Prof. Charles Sedgwick Minot on "Knowledge and Practice," one of the central sentences being:—"Our greatest discovery in scientific teaching is the discovery of the value of the laboratory and its immeasurable superiority to the book in itself." Other points are the insistence on biology as an essential introduction to the study of modern

medicine, and the inculcation of the value of the comparative method, not in anatomy alone, but in physiology, pathology, embryology, and further.

Nature for July 6 has an interesting review of the latest work on Mammalian distribution :—"The Geography of Mammals," by W. L. and P. L. Sclater,—a work which should also have been sent to *Nat. Sci.* The review is of particular interest because of the antithesis, half expressed, and half repressed, between the reviewer's conclusions and those of the authors, an antithesis which forcibly suggests the rapid progress in this department of zoology.

Mr. L. L. Otler, a vice-president of the Selborne Society, proposes to have published "The Naturalist's Calendar or Diary," kept by Gilbert White of Selborne from January 1768 to June 1793; and subscriptions to this interesting work may be addressed to A. J. Western, Secretary of the Selborne Society, 20 Hanover Square, W. The price to subscribers is 30s. a copy, to others £2 : 2s. net.

The Geological Survey of Belgium is about to publish a "universal repertory of geological work," entitled "Bibliographia Geologica," edited by Michel Mourlon, Directeur du Service géologique de Belgique, with the collaboration of G. Simoens, D.Sc.

In *Nature Notes* for June is an article reprinted from the *Standard* newspaper entitled the "Vanishing African Fauna," which, however, contains little, if anything, that is not already recorded in Mr. Bryden's "Nature and Sport in South Africa," on which work it is apparently, indeed, mainly based.

Of more interest is a note in the same serial by Mr. R. Morley, calling attention to the very serious diminution in the numbers of a West African Guercza Monkey (*Colobus vellerosus*), on account of the persecution to which it is subjected for the sake of its beautiful and valuable skin. The Government of the Gold Coast (which is the one concerned) should intervene with a strong hand, and at once prohibit such destruction.

OBITUARIES.

CARL CLAUS.

BORN AT KASSEL IN HESSEN, JANUARY 2, 1835; DIED, JANUARY 18, 1899.

PROF. KARL GROBBEN briefly reviews[1] the life and work of the late Prof. Claus of Vienna, and gives a full list of his memoirs. The majority dealt with the Coelentera and the Crustacea, and a few with the more general problems of Biology. Of his writings that which was most widely read was the work which underwent many changes of form and title since its first (1868) publication as "Grundzüge der Zoologie," and its final (1883-1897) issue as "Lehrbuch der Zoologie." The Lehrbuch was, as Prof. Grobben informs us, Claus's "Lieblingswerk," and enjoyed an extraordinary and widespread popularity. As a teacher Claus emphasized the importance of adequate practical work, and as director of the Zoological Station at Trieste was enabled to supply his students with living material. The result was seen not only in the founding of the journal in which the present memoir appears, but also in the numerous students trained under him who now occupy professorial chairs in Austria and Germany. The personal character of Prof. Claus is summed up in the two phrases:—he was a "hervorragender Forscher" and a "lebhafter Kämpfer."

The following deaths are announced:—On June 24, at the age of 55, CHARLES WILLIAM BAILLIE, marine superintendent of the Meteorological Office, well known for his invention of a sounding machine; at Boston, from typhoid fever, W. W. NORMAN, professor of biology in the University of Texas; Dr. CARL SCHÖNLEIN, assistant in the zoological station at Naples, aged 40; Dr. THOMAS O. SUMMERS, professor of anatomy at the St. Louis College of Physicians and Surgeons, on June 19; Mr. LAWSON TAIT, the eminent surgeon, on June 13, in his 55th year, one of the earlier investigators of digestion in insectivorous plants; GIANPAOLO VLACOVICH, professor of anatomy at Padua, Italy; Prof. E. G. BALBIANI, professor of comparative embryology in the Collège de France, well known for his work on the development of insects, the conjugation of Protozoa, the rôle of the nucleus, and in many other departments; on August 16, Prof. R. W. BUNSEN, F.R.S., the illustrious Heidelberg chemist, in his 88th year; on August 1, JOHN CORDEAUX, of Great Cotes-house, Lincolnshire (born 1831), a keen ornithologist, who helped not a little to organise a systematic study of bird-migration; on August 9, Sir EDWARD FRANKLAND, the famous chemist (born 1825); on July 18, at Springfield, Ohio, Prof. H. R. GEIGER, formerly of Wittenberg College, and lately connected with the U.S. Geological Survey; on July 16, W. P. JOHNSON, LL.D., President of Tulane University, New Orleans, and a regent of the Smithsonian Institution; Mrs. ELIZABETH THOMPSON, of Stamford, Conn., a liberal patron of science, founder of the Elizabeth Thompson Fund for the promotion of scientific research.

[1] *Arbeit. zool. Inst. Univ. Wien*, xi. 1899, pp. i.-xii.

NEWS.

The following appointments have recently been made:—Mr. A. F. Stanley Kent, as professor of physiology in University College, Bristol; J. L. M'Intyre, as lecturer in comparative psychology in the University of Aberdeen; C. F. Marbut, promoted to full professorship of geology in the Missouri State University; Dr. R. Martin, as professor of physical anthropology at Zürich; J. L. North, as curator of the Museum of the Royal Botanic Society at Regents' Park; Dr. A. Philippson, privat docent in geography at Bonn, to the title of professor; Dr. W. Somerville, professor of agriculture at the College of Science, Newcastle-on-Tyne, to the new professorship of agriculture in Cambridge University; Dr. E. H. Starling, F.R.S., to the Jodrell professorship of physiology in University College, London, in succession to Prof. E. A. Schäfer now of Edinburgh; Dr. E. V. Wilcox, lately professor of zoology in the University of Montana, to a position in the Agricultural Department at Washington, where he will have charge of the zoological items in the *Experiment Station Record*; Miss H. V. Whitten, as tutor in geology in the University of Texas; Mr. D. L. Wilder, as assistant on the Iowa Geological Survey; Mr. J. H. Burkill, M.A., as Principal Assistant in the Directors' Office, Royal Gardens, Kew.

The Duke of Bedford has been elected president of the Zoological Society of London in place of the late Sir William Flower.

The degree of LL.D. has been conferred by the University of Glasgow on Mr. R. L. Jack, Government geologist of Queensland.

The degree of LL.D. has been conferred by Clark University on Professors Boltzmann, Picard, Mosso, Ramon y Cajal, and Forel, who lectured at the recent decennial celebration.

Prof. K. von Zittel has been elected president of the Munich Academy of Sciences.

Surgeon-General Sir J. Fayrer, author of the "Thanatophidia of India" and other works on snakes, has had a pension of £100 per annum conferred upon him for distinguished service.

The Baly Gold Medal of the Royal College of Physicians of London, for distinguished work in physiology in the two years preceding the award, has been awarded to Prof. C. S. Sherrington.

Prof. J. Wiesner, the well-known botanist of Vienna, has been elected a member of the Berlin Academy of Sciences.

Dr. Maxwell T. Masters, F.R.S., the well-known author of "Vegetable Teratology," etc., has been made an officer of the Order of Leopold by the King of the Belgians.

Prof. Purser's work as a teacher of physiology for the last twenty-five years at Trinity College, Dublin, is being gracefully recognised by his former pupils, who are raising funds for a "Purser medal," which will be awarded annually to the candidate showing greatest proficiency in physiology and histology in the professional examination.

It is announced in *Science* that the Berlin Academy of Sciences has given Prof. Engler a grant of 4000 marks for his botanical work.

Dr. Charles Drury Edward Fortnum, who died on March 6, left the bulk of his estate, valued at £41,247, and his collections to the Ashmolean Museum of Oxford.

Mr. George Averoff, who died at Alexandria on July 27, has bequeathed £20,000 to create an agricultural school in Thessaly, and £50,000 to the Polytechnic schools and Odeon at Athens. Among his other bequests is one of £40,000 for the revival of the Olympic games, to which he devoted a similar sum in 1896.

Science announces the following gifts and bequests:—The Medical School of Harvard University is said to have received over $100,000 by the will of the late Lucy Ellis of Boston. The California Academy of Sciences has received from Mr. J. W. Hendrie securities to the value of $10,000, which will go to form a publication-fund. By the will of the late Frau M. Jankowska of Warsaw, the Academy of Sciences at Cracow receives 20,000 roubles. O. Hölterhoff, a banker, has bequeathed about 1,000,000 marks to the University of Bonn.

The supplementary vote of £65,000 required to bring about the housing of the University of London in the Imperial Institute having been agreed to, and the formal concurrence of the parties concerned having been obtained, the *matt* problem of structural adaptation is now being considered.

The University Court of St. Andrews has adopted a scheme for training candidates with a view to the Indian and Home Civil Services, which have again been brought more within the reach of Scottish students by the recent raising of the age limit. Lecturers in political economy, Sanskrit, ancient history, political philosophy, etc., have been, or will be, appointed.

We quote from *Science* the following interesting note:—Twenty-two per cent of the professors in the German universities are engaged in lecturing or laboratory supervision 2–6 hours a week, and fifty-one per cent from 7–12 hours. Of the associate professors sixty per cent are engaged from 2–6 hours per week, and of the privat docents eighty-two per cent. Only four per cent of all privat docents are engaged in lecturing or laboratory supervision more than 12 hours a week. This relative leisure may account in part for the great amount of research work done in German universities.

The summer meeting of University Extension Students at Oxford in August was attended by about 1000 students, including about 180 foreigners; and University Extension work in England is reported to be prospering.

Science notes that during the past summer session there were 4997 students matriculated at the University of Berlin, 349 more than in 1898, and including 655 foreigners.

It is reported that the number of candidates last July for the Bachelor's degree in Science was, for the first time in the history of the University of London, much greater than the number presenting themselves for examination in Arts. This interesting change is attributed to the increasing demand for science teachers in schools and colleges.

In a letter to the *Times* of August 15, Professor Raphael Meldold expresses the views of many interested in the advancement of scientific education when he calls attention to the real danger involved in the inadequate representation of science and of scientific interests among those in authority. "If the direction of the science teaching in secondary schools is at this crisis allowed to fall into wrong hands the progress of the country will be retarded for generations."

It is announced that at the seventy-first meeting of German naturalists and physicians at Munich (September 17–23) lectures will be given by Dr. Nansen on the results of his expedition, and by Prof. Chun on the German Deep Sea Expedition. Profs. Marchand and Rabl will discuss the relation of pathology to embryology.

On August 9 Professor V. Pritchard opened the International Otological Congress with an inaugural address on the history and recent advances of otology, and the retiring president, Prof. Grazzi of Florence, also gave an address.

At the annual meeting of the Royal Botanic Society on the 10th, the Duke of Teck was re-elected president. The number of new fellows and members joining during 1898 was 108, and since the beginning of this year 165 have been elected. The total number of fellows is 2102, but the society is reported to be still struggling against the common malady of too small an annual income.

The second annual "Summary of Progress" of the Geological Survey records the revision and extension of the maps of various districts. With regard to results, special attention is directed to the researches among the younger granites of the Highlands, the numerous Cambrian fossils found in Skye, the discovery of more new fishes in the Upper Silurian rocks of Lanark and Ayrshire, the evidence of the existence of volcanoes in Somerset belonging to the time of the Carboniferous Limestone, the new light thrown on the structure and probable extension of the North Staffordshire coalfield, fresh information as to the volcanic history of the western mainland of Scotland and the Inner Hebrides, and further data as to the successive stages of the Ice Age.

On Saturday, September 9, the Geologists' Association makes an excursion to Charlton, Erith, and Crayford, and on September 11 to the British Museum, Jermyn Street Museum, and Natural History Museum.

Dr. L. L. Hubbard has resigned his position as state geologist of Michigan.

We learn from *Science* that the excursions of advanced students of natural science, *e.g.*, at present of geological students, to Arizona and New Mexico, are reckoned as a regular part of the University work in Chicago.

We learn from *Science* that the State Zoologist of Minnesota, Prof. H. F. Nachtrieb, has equipped a house-boat for the study of the fauna of the Minnesota and Mississippi rivers.

The Russo-Swedish Scientific Expedition to Spitzbergen has established winter quarters at Horn Sound. Later on they will proceed by land to the western side of the Stor Fiord where they will engage in geodetic work.

The *Belgica*, with the members of the Belgian Antarctic Expedition on board, left Buenos Ayres for Europe on August 14.

Henry G. Bryant of Philadelphia, who led a search party for Lieut. Peary a few years ago, is about to attempt an ascent of Mount Assiniboine.

Prince Johann Lichtenstein has given the Vienna Academy of Sciences 25,000 florins for explorations in Asia Minor.

The Arctic Club of America, we are told by *The Scientific American*, was organised in New York in 1894, with Prof. W. H. Brewer as president, to promote a live interest in Arctic matters and to disseminate accounts of the results of expeditions. "The club has a banner of its own, which is now being borne toward the North Pole by Lieut. Peary, Walter Wellman, and others."

The slightly cracked specimen of the egg of the Great Auk sold by auction in July at Stevens's Rooms in London realised 300 guineas; a carefully made model should cost under three shillings.

At a meeting of the Royal College of Physicians on July 27 the president awarded the Bisset Hawkins Gold Medal to Dr. James Burn Russell, M.D., LL.D. Glasg., medical adviser of the Local Government Board of Scotland, and late medical officer of health for the city of Glasgow. This is the first award which has been made of this medal, which was founded in 1896 in memory of the late Dr. Francis Bisset Hawkins, to be given triennially to a medical practitioner, being a British subject, who has during the preceding ten

years done such work in advancing sanitary science or in promoting public health as in the opinion of the College deserves special recognition.

The structural alterations which have to be made at the Imperial Institute in order to provide a home for the University of London will cost £7000. The Treasury minute showed, among other arrangements detailed in it, that the Government would pay off the mortgage of £40,000 on the Institute building, and also discharge the floating debt of £15,000. This accounts for £62,000 of the vote of £65,000 which was agreed to. The remaining £3000 is for the half-year's maintenance and repairs, with fuel, lighting, and necessary furniture.

Dr. Henry Woodward of the British Museum (Natural History) has been granted an additional term of service for two years by the Treasury. This dates from October next, and is the second time Dr. Woodward has been so privileged.

The glazing of the great sauria in the gallery of fossil reptiles at the British Museum (Natural History) is now fast approaching completion. The space gained by the alteration made in this gallery is considerable, and with the exception of the upper four feet no trouble is caused by the reflection of light. Some slight alteration of the blinds will no doubt easily make the whole perfect. Below the frames of sauria is a bare space of some few feet, and this we presume will be utilised for table cases in the early future.

The Geologists' Association of London issued the usual annual pamphlet in connection with the long excursion to Derbyshire. This consisted of advance copies of the number of the *Proceedings* which will be issued at the end of the month, and forms one of a series of valuable treatises on the local geology of this county. The district dealt with includes the north and north-west portions of Derbyshire, and roughly coincides with the whole of the High Peak Division and the northern half of the Western Division of the county. The subjects included are mountain limestone, Yoredale rocks, millstone grit, sands and fire-clays, glacial drift, infas, igneous rocks, and there is a special chapter on petrology. Mr. H. H. Arnold Bemrose is author, and was also principal director of the excursion; the pamphlet can be had from the secretary for the usual eighteenpence.

CORRESPONDENCE.

DEAR SIR—I shall be glad if you will correct an impression which may be conveyed by a partial quotation from the evidence of the Select Committee of the Cape Parliament on Trawling, and appearing in a recent number of your valuable paper. The full quotation is: "the *evidence*" (*i.e.* of the fishermen examined) "has shown that we know absolutely nothing about the spawn of the fish, or very little."—Yours truly, J. G. F. GILCHRIST.

DEPARTMENT OF AGRICULTURE, CAPE OF GOOD HOPE,
CAPE TOWN, 21*st June* 1899.

[We regret that our colleague who reviewed the paper referred to appears to have misunderstood the sentence.—ED. *Nat. Sci.*]